

JAMES AND THEODORA BYNON (ED.)

# HAMITO-SEMITICA

PROCEEDINGS OF A COLLOQUIUM HELD BY THE  
HISTORICAL SECTION OF THE LINGUISTICS  
ASSOCIATION (GREAT BRITAIN) AT THE SCHOOL  
OF ORIENTAL AND AFRICAN STUDIES, UNIVER-  
SITY OF LONDON, ON THE 18TH, 19TH AND  
20TH OF MARCH 1970

MOUTON





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edited by

JAMES AND THEODORA BYNON

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## FOREWORD

Members of the Linguistics Association who did not take part in the 1970 Hamito-Semitic Colloquium may view its success with the slightly awed satisfaction of parents who have launched a brilliant child on a notable but perhaps no longer comprehensible career. The Association decided to sponsor the Colloquium in 1968, when it was suggested by Drs. J. and Th. Bynon. Its evident success and the important contribution that it has made to Semitic and allied studies are the result and reward of their indefatigable work as organizers. We owe concurrent gratitude to the School of Oriental and African Studies of the University of London for the financial and other practical help which made it possible for so large a number of members to take part, particularly so many from abroad. It was a special honour for us that Professor I. M. Diakonoff contributed the opening address and that Professor Marcel Cohen joined us and gave the introductory paper.

The Editors' preface will assess the achievements of the Colloquium in its special field. This Foreword may appropriately suggest what it has contributed for the methodology of comparative and historical linguistics in general. In this branch of linguistics, assumptions and ideas about method may still be too much influenced by comparative study of the Indo-European languages, which may have atypical problems to investigate. If so, the Semitic and 'Hamitic' languages may provide a useful corrective. They are known over an even longer time-span than the Indo-European. But they include a larger proportion of languages, among them whole groups or sub-families, which have no written tradition. Comparison of these will be a particularly 'pure' linguistic exercise, since it will require systems in putatively related languages to be compared, without any possibility that the data in individual, currently spoken languages may be interpreted in the light of the characteristics of one or more others which are older but less well-attested and less open to structural analysis and statement. Such comparison may indicate the minimal criteria on which assumption of genetic relationship in languages may be based. Here, studies of similarities in typology or 'deep structure', perhaps reinforcing slender similarities in lexis or formants, should be particularly important. A special problem is that of making allowance in comparison for the effects of substrates on the systems of

individual languages or of sets which may be genetically related. Work on 'Chadic' and 'Cushitic' languages makes these problems specially plain; and K. Petráček has given us a notably important treatment of the problems of comparing 'actual languages' with 'constructs' based on sub-groups, within a programme of comparison whose aim is to establish or disprove genetic relationship within a larger set of languages or groups. B. Adams' paper reminds us that when features and systems of substrates can be stated, comparison of them might indicate the distribution of prehistoric languages or of typologically similar sets.

A problem of general interest which has been stated with new clarity is that of the relative significance of similarities in morphology, semantics and other features in determining 'degrees of relationship' among languages, for example in the papers of Carleton T. Hodge, J. Vergote and W. W. Müller. Professor Hodge notes the importance for definition of language-families of determining whether there are semantic and syntactic 'universals', or regional norms of wide incidence, whose occurrence in individual languages will not be significant. A valuable and welcome experiment is reported by M. L. Bender: his attempt to determine empirically whether the idiolects of individuals classed as speakers of dialects of one language were mutually intelligible or not.

Lexicostatistics and glottochronology have received due attention. The vocabularies of Egyptian-Coptic and Arabic, with their exceptionally long periods of attestation, might be expected to provide ideal material for testing the glottochronologists' thesis that rate of change in vocabulary is standard over a period of longer than one millennium. Egyptian and Coptic do not appear to have been studied as a continuum in this respect, as regards their lexis. But one suspects that both they and Arabic will show the conservative effects of their role in literary and religious traditions and in administration. In general, the factors which seem likely to make glottochronological calculation unreliable have been emphasized, e.g. by Chaim Rabin. On the other hand, the plausible results of P. Fronzaroli's attempt to deduce from the lexicon which may be set up for Common Semitic the material culture and environment of the people who spoke it are encouraging for those who hope that linguistics may help in reconstructing prehistoric events and cultural patterns.

Current thinking about utilization of the results of linguistic comparison in study of prehistoric periods was presented and discussed, in particular by B. Isserlin and C. B. M. McBurney. *Prima facie*, there would seem to be a better chance of suggesting a likely 'region of origin' for 'proto-Hamito-Semitic' (or whatever other corresponding *Ursprache* may be posited) than for Indo-European. There seems less need to postulate rapid, long-distance migrations at the end of prehistoric periods to explain the distribution and degree of difference of Semitic and 'Hamitic' languages when first known than there is in the case of the Indo-European languages. The problem of determining the circumstances in which community of language is likely to correlate with cultural or genetic (ethnic) similarity among human groups remains difficult. Specially valuable in this connection was discussion of the kind of



cultural traits which human communities tend not to borrow from each other unless they amalgamate. If identified, these could be particularly useful indicators of movements of whole coherent communities.

Since convergence is a popular concept in linguistics at present, it was interesting and agreeable for the organizers to learn early in 1969 that a *Premier Congrès International des Études Chamito-Sémitiques et Sémitiques* had been arranged for June of that year, in Paris. It would not have been specially appropriate for the Linguistics Association to have considered sponsoring a second Hamito-Semitic Colloquium after the first in 1970. So it is welcome news that such a meeting is planned for 1974, in Florence. It will continue the work of both the Paris congress and our own Colloquium, and the Association looks forward to what it will achieve for general linguistics as well as for Hamito-Semitic with strong and empathic interest. Finally, all members of the Association, and linguists in general, will owe a second debt of gratitude to James and Theodora Bynon for seeing this volume so ably through the press, and to Messrs. Mouton for assuring its publication.

R. A. CROSSLAND,  
*Chairman of the Historical  
Section of the Linguistics  
Association.*



## PREFACE

The present volume is the outcome of a three day Colloquium held in March 1970 at the School of Oriental and African Studies. The aims of the meeting were described in the First Circular sent out to prospective participants, inviting papers "... dealing with the internal examination of one of the five main language groups which have been put forward as members of the postulated language family, namely Semitic, Ancient Egyptian, Libyco-Berber, Cushitic and Chadic". Within each of these groups it was suggested that contributions should concentrate on answering three basic questions:

- (1) How strong is the evidence for postulating such a genealogically related group, that is to say a group made up of the descendants of a common proto-language?
- (2) What can be reconstructed of the phonology, grammar and lexicon of the proto-language of the group?
- (3) What can be deduced about the ecological and cultural background of the speakers of the parent language from an examination of the proto-lexicon?

During the Colloquium the broader issues of possible inter-group relationship would be discussed.

Two main concerns governed the organizing of the actual meeting. In the first place, it was felt that the reading out of papers is a time-wasting operation which does not really allow adequate opportunity for considered reaction by the audience. All papers were therefore duplicated and distributed to participants by post well in advance of the meeting. Secondly, it is our belief that one of the principal obstacles to progress in the field of Hamito-Semitic comparative studies is lack of communication between scholars in the different disciplines that make up the subject so that a wealth of detailed expert knowledge in individual fields has remained largely unexploited for the overall issue. It was therefore decided that, while the papers would be dealt with under section headings, there would be no sectional meetings—all

sessions would in fact be plenary sessions and would be devoted entirely to discussion.

The Colloquium was initiated by the two scholars who have attempted overall syntheses of the subject, Marcel Cohen of Paris and I. M. Diakonoff of Leningrad.

The extent to which the original aims of the Colloquium were in fact realized may best be judged by the reader. Happily, scholars are by nature an independent-minded breed and consistently resist or ignore attempts at direction. The proposed framework did, however, have a certain unifying effect and has we think resulted in a body of material which, while always in some way relevant to comparative Hamito-Semitic studies, ranges from broad and often stimulating suggestions to the resolution of detailed but none-the-less crucial problems.

The results are perhaps even more encouraging than we had dared to hope, especially in the Chadic and Cushitic fields. It is obvious that if there is still to be anything approaching a rapid or spectacular advance it is likely to come in these areas. It is, however, equally apparent that we cannot for much longer continue to shirk the arduous task of reconstruction, the only demonstrably reliable means of separating loans and innovations from cognates and of establishing a theoretical framework for linguistic relationships. While several of the contributions appear to constitute very promising pointers, it is nevertheless clear that we are still a long way from the *terra firma* of the Indo-Europeanists, and that a huge amount remains to be done before what may be termed 'the Hamito-Semitic hypothesis' can hope to find a positive solution—be it in the demonstration of its essential validity, or in its rejection in favour of some more adequate explanation of the undoubted resemblances which exist between the languages involved. These points of affinity, which embrace both actual grammatical morphs (personal pronouns, gender and person markers, etc.) and 'basic' lexemes of a type hardly likely to be borrowed, are too impressive to be dismissed and the issues at stake, both from the point of view of the comparative linguist and of the historian, are such that they cannot be simply ignored.

In a field as large and as complex as that of Hamito-Semitic considerable divergencies in methodological approach and in the concept of the nature of linguistic relationship are to be expected. This is a natural, even a welcome state of affairs. It should not, however, preclude the editors from themselves stating their position on these issues. In particular they would like to add their voice to those that have repeatedly stressed the lack of justification for giving special status to any individual member of the postulated family. Marcel Cohen stated in *Les Langues du Monde* as long ago as 1924, and has constantly repeated since, that there is absolutely NO justification for grouping the non-Semitic members of Hamito-Semitic together under the label 'Hamitic'. This view has been restated recently in even more vigorous terms by J. H. Greenberg, who points out the importance from the methodological point of view of classifying languages on linguistic evidence alone: "The most common source of error in this regard is the fact that prominence of a language or group of languages because of practical importance, extent of population and territory or

literary cultivation tends to lead to separate status in classification". He cites in particular "... the special status accorded Semitic in the Hamito-Semitic (Afro-asiatic) family as against the remaining four branches which were generally lumped together in a pseudo-entity Hamitic" and concludes "... the linguistic use of the term Hamitic should be abandoned. The Semitic languages do not occupy any special place in the total Hamito-Semitic complex. Their cultural importance and connection with our own historic past has led to a separate treatment which is not justifiable linguistically. In other words, the non-Semitic languages of the Hamito-Semitic family do not form a linguistic unity as against Semitic. Therefore, the term Hamitic, which has been reserved for this use, does not refer to any valid linguistic entity" (*The Languages of Africa*, 1966, 4, 50). Our own standpoint in this matter could not be expressed with greater clarity.

It is perhaps worth noting that parallel situations have arisen on more than one occasion in the Indo-European field. Thus the inability to find criteria in justification of a 'European' branch of the family tree was ultimately to lead to its radical revision (NOT, however, accompanied by a renaming of the language family as a whole!) and again, more recently, the prominent role accorded Greek and Sanskrit in the reconstruction of the morphological system of the proto-language had to be abandoned following the discovery of Hittite. The temptation to make particularly well and early documented languages, especially when in addition they share much of their morphological structure, the standard of comparison is understandable enough. It did, however, in the case of Indo-European hold up the reconstruction of a more adequate proto-system having EQUAL explanatory power with regard to the grammars of ALL the descendant languages.

This explanatory capacity of the comparative method raises a further issue. Recurrently attempts are made to account for the shared features of Hamito-Semitic languages as being the result of the super-imposition of 'Semitic' traits upon originally unrelated neighbouring 'African' languages by some process of diffusion. The drawback to such an approach, apart from the fact that there is no means of verifying its claims, is its inability to explain particular phenomena and its failure to generate new information. The only model of language evolution which has so far proved itself to be productive in these respects is the one inspired by the Neogrammarians, in which the grammars of descendant languages are derived from that of the ancestor language by ordered sets of changes or 'rules'. When comparing the various theoretical approaches that have been advocated, it is perhaps worth bearing in mind that mere explanatory adequacy for the immediate purpose at hand cannot in itself constitute a valid demonstration of the correctness of a hypothesis—the best test of this is its power to predict when applied to material additional to that in explanation of which it was originally postulated. This is a test of validity which historical reconstruction has shown itself amply capable of meeting in the Indo-European field.

These rather theoretical considerations do not imply that there is any simple solution to the very real problem of separating inherited from 'areal' phenomena

resulting from language contact. The comparative method does, however, appear to provide reliable evidence of genealogical relationship when this exists and a sound framework from which to assess contact phenomena.

In conclusion, then, we feel that in spite of differences in outlook and in theoretical approach, this coming together of scholars sharing a common interest in the Hamito-Semitic question has been academically useful as well as a very pleasant social occasion, and we would like to thank both the School of Oriental and African Studies for providing hospitality and a meeting place and those of our colleagues who helped us in the planning and organization of the Colloquium, notably the Section Conveners and T. W. Thacker of Durham who, although unable to be present at the actual meeting, gave us invaluable advice during the initial planning stages. We also acknowledge gratefully the advice of colleagues in the preparation of the papers for the printer.

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# **I**

## **OPENING SESSION**



## QUELQUES MOTS SUR COMPARAISON ET RESTITUTION

MARCEL COHEN

Je veux d'abord exprimer mon plaisir d'être ici et féliciter notre collègue Bynon d'avoir eu l'idée, avant que nous ayons un congrès de Paris, d'organiser un colloque chamito-sémitique. Je me réjouis aussi sans modestie du succès que nous avons eu à Paris pour le congrès qui s'est appelé « Études de linguistique chamito-sémitique et sémitique », et nous attendons, je le dis tout de suite, avec espoir le premier congrès qui aura lui le nom de chamito-sémitique dans son ensemble international : chamito-sémitique ou sémito-chamitique, je ne sais pas ce que nos collègues italiens qui sont chargés de l'organisation choisiront.

Je suis, il faut le dire, embarrassé pour ce que j'ai à vous dire. Je n'ai pas voulu me refuser à prendre la parole mais je ne pouvais pas préparer une véritable communication dans l'état où je suis maintenant de privation de la lecture. Je vois encore mais je ne peux pas lire. C'est un état assez désagréable que je ne souhaite pas à d'autres de connaître; mais je ne suis pas le seul, malheureusement.

Je dois encore le dire en commençant, j'ai connu dans ma carrière de chamito-sémitisant où j'essayais d'aller de l'avant certaines déconvenues. Après que j'avais publié le livre que vous connaissez, l'essai de phonétique et de comparaison étymologique chamito-sémitique,<sup>1</sup> j'avais espéré convaincre définitivement ceux qui pouvaient être hésitants en ajoutant aux comparaisons morphologiques que nous connaissions depuis longtemps et qui, je voulais le croire, avaient parues convaincantes, à peu près cinq cent rapprochements de vocabulaire. Je n'ai pas eu le succès que j'espérais, je dois le dire; mon maître Meillet, qui m'avait toujours encouragé auparavant, a dit que finalement il ne pensait pas que le chamito-sémitique soit une unité comparable à l'indo-européen, c'est-à-dire au point de vue des relations de famille, celles d'un idiome ou d'un petit groupe d'idiomes partant d'un point unique que nous ignorons pour aboutir aux langues qui se sont répandues sur un si vaste espace dans l'extrémité occidentale de l'Asie et dans une partie de l'Afrique. Quelqu'un d'autre que je ne veux pas nommer a dit que finalement il ne savait pas très

<sup>1</sup> *Essai comparatif sur le vocabulaire et la phonétique du chamito-sémitique* (Paris, Librairie ancienne Honoré Champion, 1947; reprinted 1969).

bien ce que pouvait être le chamito-sémitique — ceci à un moment où j'étais en question pour l'occasion, une dernière occasion d'obtenir une chaire de linguistique chamito-sémitique. J'ai réfléchi, naturellement, à ce qui se passait et je me suis demandé si une circonstance en particulier n'était pas responsable de ces attitudes. C'est de ça que je veux vous parler très brièvement en forme de causerie de quelques minutes.

Eh bien, c'est que je n'ai pas employé de formes restituées avec astérisque. Les comparatistes indo-européens ont essayé d'imaginer des formes primitives — pas primitives absolument parce qu'elles avaient forcément un passé derrière elles, mais enfin des formes très anciennes — et de les munir d'un astérisque pour désigner ces formes en somme reconstituées. C'est pourquoi j'ai employé pour l'annonce de cette communication le terme de comparaison et de reconstruction, on peut dire RESTITUTION. Je ne nie pas que cette méthode n'ait eu de très bons résultats, qui sont d'ailleurs assez mal catalogués parce que je ne suis pas arrivé à faire dire à mes collègues qui s'occupent de l'indo-européen sur combien de restitutions tout à fait vraisemblables ils appuyent leur grammaire comparée — il y a des affaires de statistique à perfectionner. D'autre part je ne me repens pas du tout et je continue à penser qu'il est utile, qu'il est nécessaire de faire de la comparaison, mais qu'il est inutile de matérialiser les comparaisons dans des reconstructions qui sont le plus souvent imprudentes. Et ici je voudrais nommer un homme dont l'influence n'est pas assez connue. On doit bien dire dans l'histoire de la linguistique au vingtième siècle que s'il y a eu la révélation Ferdinand de Saussure qui a eu tous les prolongements que nous connaissons et qui en aura encore d'autres, il y a eu aussi l'œuvre de Jules Gilliéron qui a créé ce qu'il appelle lui-même la géographie linguistique.

Gilliéron n'a pas eu la première idée mais il a fait la première exécution d'une grande exploration linguistique sur le terrain, celle de la France, et l'examen de l'*Atlas linguistique de la France* a donné d'étonnants résultats. En ce qui concerne l'histoire des mots, on a vu, on a pu voir, on peut voir encore en étudiant les cartes, combien l'histoire des mots est compliquée et souvent inattendue, et je crois que ça doit décourager les vues simplistes. Gilliéron lui-même a traité de ce sujet. Il faut lire ses opuscules intitulés *La faillite de l'étymologie phonétique* (1919), *Les étymologies des étymologistes et celles du peuple* (1922) et son livre *Généalogie des mots qui ont désigné l'abeille d'après l'Atlas linguistique de la France* (1918). Il y a une référence que je n'ai pas pu retrouver mais qui est profondément fichée dans ma mémoire : à propos d'une reconstruction d'un mot de bas latin qui aurait résisté et dont on n'a pas d'attestation, il a parlé pittoresquement d'un nénufar du Sahara. C'était bien son idée qu'il ne fallait pas se livrer à ces sortes de reconstitutions, et j'empiète ici sur les domaines qui ne sont pas le nôtre, en m'adressant aux indo-européistes et aux romanistes. Il se trouve que nos dictionnaires étymologiques sont pleins de formes à astérisque que je dirais pour le moins imprudentes. C'est ainsi que le latin vulgaire, dont je parlais tout à l'heure, est orné, enrichi, de toutes sortes de mots qu'il n'a jamais possédés, et qu'on a reconstitué tout un vocabulaire de francique,

c'est-à-dire la langue des Francs qui ont à un moment donné envahi une partie de la Gaule alors qu'on n'a aucun document permettant d'affirmer que ces mots ont existé.

Pour nous, pour le chamito-sémitique, je prendrai seulement un exemple, dans la morphologie du sémitique. Eh bien, je n'oserais pas choisir en ce qui concerne la forme de l'accompli du sémitique occidental, entre les formes de la deuxième personne *katabta* et *katabka*, l'une étant attestée par l'arabe et l'hébreu, l'autre par l'ensemble éthiopien. Je crois qu'il faut mettre en parallèle les deux paradigmes et montrer leur parenté évidente mais qu'il est imprudent de choisir d'un côté ou de l'autre. Et de même en ce qui concerne le vocabulaire, je ne donnerai qu'un exemple. C'est un des noms du cœur qui est *lebb* en éthiopien; nous avons là une correspondance curieuse parce qu'en face de *lebb* sémitique il y a en égyptien *yḅ*, quelque chose que nous supposons avoir été *yebb*, et nous avons des preuves de la correspondance régulière de la liquide *l* avec la sémi-voyelle *y* en égyptien. Ceci ne fait pas grosse difficulté. En couchitique nous avons des formes *lebb* qui ressemblent au sémitique, je dirais qui ressemblent TROP au sémitique parce que nous n'avons pas la preuve qu'il n'y a pas eu un emprunt ancien, et c'est assez extraordinaire que l'ensemble couchitique nous donne tout à fait la même chose que le sémitique. Je dis tout à fait en ce qui concerne au moins le consonantisme. Ici je voudrais parler de l'exemple de quelqu'un dont nous déplorons l'absence : c'est Diakonoff, dont nous allons entendre tout de même une communication, qui a eu le mérite en ce qui concerne les rapprochements de vocabulaire qu'il a fait dans son petit manuel — premier manuel de chamito-sémitique que nous ayons — de ne pas mettre d'astérisques et de ne pas mettre de formes reconstituées de chamito-sémitique. Mais il s'est laissé aller tout de même à employer l'astérisque pour le sémitique et il a mis *\*libb*. Qu'est-ce qui nous prouve que c'était *\*libb* et non pas *\*lubb* ou peut-être *\*labb* ou *\*lebb*? Nous n'en savons absolument rien, et je ne vois pas l'avantage de mettre cet *\*i*. Je continue mon propos : dans le berbère qui est si varié dans ses dialectes, dans ses manifestations il n'y a qu'une forme pour appeler le cœur : *ul*. C'est une beauté de la comparaison de pouvoir dire que *ul* égale *lebb* et là ma conviction est absolue autant que celle de Diakonoff et celle je suppose de beaucoup d'autres; *ul* est bien le même mot à l'origine que *lebb*, mais par où les choses ont-elles passé pour que *lebb* devienne *ul*, si vraiment *lebb* est plus ancien que *ul*? Voilà une question.

Je répète donc : je crois qu'il est plus prudent de mettre côte à côte les formes comparées dont la comparaison est valable et féconde et de ne pas trop s'imaginer qu'on puisse reconstituer la forme antérieure.

Il y a encore un point que j'ajouterai, c'est que nous n'avons réellement pas d'hypothèse vraisemblable pour l'existence d'un proto-chamito-sémitique et pour sa situation géographique. Pour l'indo-européen on s'accorde pour dire vaguement qu'il a dû y avoir un réservoir d'hommes quelque part aux limites de l'Europe et de l'Asie. Mais je crois que la question n'est pas établie et c'est très vague, et Meillet dont je vous ai parlé a eu le mérite de publier un petit ouvrage sur les dialectes indo-européens en disant justement que probablement on ne peut pas remonter à une langue unique

mais à un groupe, à un magma de dialectes voisins les uns les autres. J'avoue que pour mon compte je n'imagine pas où a pu être le réservoir d'où seraient sortis, d'où sont sortis d'une part l'ensemble sémitique, d'autre part la partie chamito-sémitique de l'égyptien qui certainement comporte beaucoup d'éléments africains, et le berbère dont nous ne connaissons absolument pas le substrat antérieur, et toute la variété des langues couchitiques qui rejoignent plus ou moins par des tentacules certaines langues africaines. Si le tchadien doit réellement nous être adjoint (je crois qu'il doit nous être adjoint mais non pas incorporé) la question se pose aussi : comment le tchadien s'est-il formé?

Je souhaite à vous tous qui continuerez les études beaucoup de comparaisons fructueuses et je vous souhaite aussi des connaissances plus approfondies en ce qui concerne l'anthropologie des peuples qui ont parlé et qui parlent encore des langues chamito-sémitiques. Il y a quelque chose de frappant, c'est que dans le Tchad en question il y a bien des langues tchadiennes qui sont en litige mais il y a aussi de l'arabe, et l'arabe forcément vient d'Arabie; il est venu d'Arabie à une date récente par des voies absolument ignorées. Les gens qui ont émigré, qui ont peuplé certaines parties du Tchad n'ont pas envoyé de communications télégraphiques pour dire qu'ils étaient arrivés et qu'ils allaient continuer à parler l'arabe. Donc je crois que nous sommes encore dans l'enfance de nos études, une enfance que je pense vigoureuse, mais enfance tout de même, et c'est ainsi que je vous souhaite de rendre notre étude de plus en plus adulte.



## OPENING ADDRESS\*

I. M. DIAKONOFF

It is with great hesitation that I set about stating my thoughts on the general problems of Semito-Hamitic (or, better, Afroasiatic) linguistics because, in spite of having written two or three papers on the subject, I have all my conscious life been, and actually still am, an outsider in this field. If I have nevertheless decided to impart the following ideas to my colleagues it is because, after some consideration, I saw that my position of half expert and half outsider is on the present occasion accompanied by a certain advantage, namely that of seeing things from the outside while at the same time not entirely misunderstanding the inner problems which exist in this particular field of scholarship.

Another reason why I am perhaps in an advantageous position for discussing general problems of Semito-Hamitic linguistics is that, although in my time I graduated as a linguist and have in the last decade been mostly engaged in linguistic research, studying several languages of very different families, my real interests have always lain in the field of history and it is actually the HISTORICAL aspect of linguistics which has always appealed and still appeals to me. Now if we are to discuss the GENERAL problems COMMON to the whole Semito-Hamitic linguistic family this means, if I am not entirely mistaken, that we must discuss what really UNITES all the languages in question, that is precisely the genetic and historical questions. This is because if we were to discuss only the particular problems of each individual language as it exists at the present stage, we could just as well do so within the narrow circle of our nearest colleagues—Semitists, Berberists, Egyptologists and so on. We would not need to compare notes with students of other Semito-Hamitic languages any more than with other linguists engaged in studying typologically similar minor problems.

Thus we should at this point, as I see it, discuss the possibility of creating a historical grammar of the Semito-Hamitic language family, emulating in this task our elder and more experienced brethren in the Indo-European field. As a historian, and as a linguist somewhat familiar both with some Indo-European dialects and with the

\* Read *in absentia*.

archaic languages of the Ancient Near East and the Caucasus, I have perhaps a clearer view of the historico-linguistic problems facing us than I would have were I not half an outsider. Let this, then, be my excuse for the present address—and also for this rather lengthy introduction to it.

The starting point of my reasoning must be the Semitic branch, firstly because it is the branch of Semito-Hamitic which has been by far the best investigated and secondly because I am, of course, myself a Semitist. One of the reviewers of my booklet *Semito-Hamitic Languages* (1965), Dr. W. W. Müller (1968, 366), has very appropriately quoted Theodor Nöldeke who, when replying to the pedantic caution of certain other Semitists who warned their colleagues against venturing upon the unexplored and, as it were, alien soil of Africa, told them “dass sie aus dem semitischen Material nicht zu viel folgern mögen”. If this were true in Nöldeke’s time, then its truth is even more glaring today. A Semitist who tries to reconstruct the genesis and the history of the different linguistic categories which exist in Semitic on the basis of a study of Semitic only, can in fact achieve not only very limited, but to a considerable extent not even very trustworthy, results. To study the linguistic facts of one particular branch of a given linguistic family while ignoring the facts of the genetically connected branches of the same family is surely a dangerous procedure. This has been amply shown by research in the field of Indo-European linguistics, where not only the history of the Iranian languages cannot be studied with profit without referring to the Indian languages, but also the Indo-Iranian languages as a whole show a number of features inexplicable without reference to Germanic, Slavic, Hittite, etc. The risk incurred in studying the Semitic languages while ignoring the data of the other four branches of the same family can be particularly clearly illustrated from the BEST work recently carried out in the historical linguistics of this field. Among the most important works of general significance for Semitic studies I would place the monograph of J. Kuryłowicz, *L’apophonie en sémitique* (1961) and especially his little paper “Esquisse d’une théorie de l’apophonie en sémitique” (1958) which I must confess I like better, and the book by I. J. Gelb, *Sequential Reconstruction of Proto-Akkadian* (1969). I think I shall take Gelb’s book for my present illustration precisely because Professor Gelb is my personal friend and is one of the modern philologists whom I most admire. I do think that Gelb’s *Sequential Reconstruction* starts an entirely new epoch in Semito-Hamitic linguistics, and this is why I hope that there will be no ill feeling on the part of the author if I select his book for my criticism.

As every Semitist today probably knows, Gelb has demonstrated with the utmost clarity that all Semitic morphemes are ‘sequential morphemes’ because they appear in a certain ordered sequence. In analyzing this sequence each segment of a speech unit must be accounted for and its form and function determined; the markers for each segment must be denoted in their proper sequence within the speech unit. The order of the sequential morphemes is absolute and immutable. Among them, gender can never occur after number, nor number after case or mood, nor case after

object, nor object after enclitics. The morpheme markers are either overt or covert, in this latter case the marker being either 'zero' or 'omission'. Certain apparent, although not real, inconsistencies in the sequential order are to be found in cases where a secondary marker has been added to a primary marker for the purpose of strengthening the morpheme. The whole system displays itself in the existence of pairs of semantic opposites, one place or rank being assigned to each pair in the sequential chain.

It is easy to demonstrate that Gelb's scheme is fully applicable to the history of any Semito-Hamitic language, at least at some reconstructed stage of its development, and that it explains with admirable lucidity the basic grammatical structure of Semito-Hamitic. All the more significant therefore are those instances where Gelb's statements are not borne out by the Semito-Hamitic material outside of Semitic proper, or when they do not take into consideration the probable history of the linguistic structure.

Thus according to Gelb proto-Semitic had two markers of gender, *u* for masculine and *a/i* for feminine. The consonant *t* does not, according to him, form part of the feminine marker but is a consonantal glide introduced secondarily between the vowel of gender and that of case. The vowel *u* as a masculine gender marker had not previously been noted by Semitists, but Gelb's discovery is brilliantly corroborated by both the Cushitic and the Berber data. What, however, of the feminine marker? If *t* were secondary, we would expect it to be absent in at least some of the branches of Semito-Hamitic which, however, is not the case. It is preserved as a marker of feminine gender in all the branches, sometimes in its original form, sometimes as some survival only but always to be reconstructed with confidence. The stress that is laid at the present day on phonology must not make us forget that there exists such a thing as phonetics which reposes upon human physiology—one cannot postulate a consonantal glide *t* without giving any explanation of its physiological mechanism. The evidence adduced by Gelb (1969, 3. 3) is not conclusive because it refers to sporadic and not to systematic phenomena.<sup>1</sup>

Similarly, Gelb's reconstruction of the original forms of the personal pronouns needs to be checked by comparison with data drawn from non-Semitic members of the Afroasiatic language family.

Another correction to Gelb's scheme which I would suggest is the following. According to him rank no. 3 after the stem is occupied by the case markers. The binary opposition here is nominative *u*: genitive/accusative *a/i*. The triptotic system is a later one. With this I would concur on principle; however, a reconstruction of the earliest proto-Semitic-Hamitic phonological system seems to show that, although

<sup>1</sup> It appears to me that Gelb is right when he postulates a feminine gender marker *a* opposed to a masculine gender marker *u*. As regards *t*, this probably belonged originally to another system of nominal class markers of the type suffixed *-b*, suffixed *-r*, suffixed *-l*, these others having been early lexicalized. The same fate would have overcome the *t* suffix had it not acquired the secondary rôle of boundary marker between the new gender marker *a* and the case markers.

originally there actually were in Semito-Hamitic only two vowels these vowels were not *u* opposed to *a/i*, but *a* opposed to *u/i*. The original binary opposition must, therefore, have been *u/i* : *a*. This is easily explainable if we do not regard the present-day 'nominative construction' of the sentence in Semitic languages as immutable. As I have attempted to show on other grounds, the construction of the Semito-Hamitic sentence with a fientive predicate was originally 'ergative', involving the opposition of an oblique case (expressing, among other things, the subject of action) and an absolute case (expressing the subject of a state, which includes also the subject of a state resulting from an action, that is to say in our present-day terms, the direct object of the action). I have also tried to show independently that the marker of the absolute case was *Ø/a* and the marker of the ergative case was *u*<sup>2</sup>—but in Egyptian probably *\*i*—which in the other languages developed into a special genitive case. This is why all predicates in Egyptian are formally nouns, with their logical subjects coinciding with genitival attributes. Thus my reconstruction is, in principle, fully in keeping with Gelb's scheme but makes allowance for a historical development of such important elements of language as the 'subject : object' relation and agrees with independently acquired evidence concerning the original phonological system of Semito-Hamitic.<sup>3</sup>

Of course there always will be minor points of dissent such as these, which inevitably arise in connection with any work which opens up entirely new prospects for research. The comparison of Akkadian data with other Ancient Semitic and Semito-Hamitic material will probably also lead to minor modifications of Gelb's brilliant idea, without his main principles of approach being in the least shaken. But the example of Gelb's work shows plainly enough, I hope, that any reconstruction of the development of a given language or branch of languages will need to be checked and corroborated by comparing it with the data of the other branches of the Afroasiatic language family, while the possibility of an inner historical development of the grammatical 'logic' of the language must also always be kept in mind.

I think my point must be sufficiently clear. The same type of criticism may be applied to some of Kuryłowicz's work, or the work of any other scholar who tries to reconstruct the history of Semitic (or, for that matter, Egyptian or Berber) without paying attention to the evidence of the other branches of the language family. The

<sup>2</sup> I am aware of the fact that both Gelb and one of my reviewers, D. O. Edzard, object to my identification of the Akkadian locative in *-um* (or *-ūm*, according to Gelb), with the nominative *-u*. However, I think that although these certainly are different cases in historical Akkadian, their differentiation has occurred relatively late. This is corroborated by the absence of the locative in *-um* in any other languages of the family; Arabic and Ethiopic adverbs in *-u* or *-ū* are inconclusive evidence, as Gelb himself points out, and the Middle Babylonian and Middle Assyrian *plene* writings apparently indicate stress, not length (Kaplan 1965, cf. Gelb 1969, 3. 8).

<sup>3</sup> In the feminine plural the original opposition would also be *-u/i*-case : *zero/a*-case, i.e. *\*kalb-āt-u-m* : *\*kalb-āt-ø-ø*. The introduction of the genitive case in *-i* into the system probably induced the formation of the symmetrical pair *kalb-āt-u-m* : *kalb-āt-i-m*, since genitive coincided with the accusative in most forms except the singular of the substantives (and adjectives).

dangers of isolation within a single linguistic branch are apparent in all works of this kind when certain phenomena are presented as allegedly late or secondary while in fact they are attested in all or at least in several branches of the family and thus must certainly date from Common Semito-Hamitic—or when, for instance, certain other phenomena are presented as supposedly early when they are not attested in any other branch of the family. It must be stated quite categorically that shutting oneself up within one particular branch of a greater language family will have to become a thing of the past if we are to reconstruct a historical picture of the development of Semito-Hamitic as a whole or even of any one of its branches.

There does, of course, exist a very serious impediment standing in the way of the necessary course of development of Semito-Hamitic linguistics, and that is the state of study of the African branches of Afroasiatic. Not only have all the languages of these branches—with the exception of Egyptian, but then Egyptian is known only in its non-vocalized form—reached us only in their modern forms which belong with very few, if any, exceptions to the New Stage of Semito-Hamitic, but very few attempts have been made to reconstruct the historical phonetics, phonology and morphology of each branch and even the sub-grouping of the branches is very far from being clear. Curiously enough the historical and comparative aspects of, for example, Cushitic or Chado-Hamitic seem to have aroused very little interest—or at any rate much less than they deserve.

At first the reason for this was our inadequate knowledge of the languages in question—I need not tell you how many of them have not yet been described at all—but in the latter years there may also have been another cause, namely enthusiasm for the descriptivist techniques which have for some time been very much in vogue among linguists.

Paradoxical as this may sound, the effect of the introduction of new linguistic techniques has on the whole been unfavourable for historical linguistics. The quest for rigorousness, the war against looseness of definitions has, of course, been of the greatest value for the progress of linguistic science as a whole. All things, however, have their useful and their harmful sides and formalization in linguistics is no exception. Since linguists have in recent times forgotten that they belong within the sphere of the humanities and have tried to imitate the physicists and the mathematicians, I may here be allowed to quote a great physicist, W. Heisenberg (1949, 67), who once said: “The character of observation decides what features of nature will be defined, and what features will be effaced by our very observation”. A formalized statement has the delusive guise of finality, and when a whole tower of formulae is erected by a logical process of transformation of the original statements based on observation, who knows how many facts and phenomena which had seemed irrelevant to the observer may have been passed over in the process of formalization and lost to science for a very long time if not forever? And, as a result of this, how far away may the final reconstructed result be from the realities of the historical situation under study? A language formalized by descriptivist techniques resembles a robot;

the robot imitates a human being's movements but lacks the natural grace of their curves because each one is broken up into a number of minor rectilinear movements. Modern linguistics is very far away from the ideal, which could be envisaged by a de Saussure, of studying language as an entity in which all the parts interact. We have reached a point where language is broken up into non-interconnected levels and we study each of these by itself, shutting our eyes to phenomena which, however evident they may be, seem to us to belong to a level other than the one we are studying at the moment in the hope that there will come a time when the results of the separate studies at each level can be put together like the parts of a jigsaw puzzle in order to produce at last a general overall picture. That time, however, will never arrive because language is an entity and its subdivision into levels is no more than a gross act of abstraction—and sometimes even an unwarranted one. In studying the levels by themselves we therefore of necessity ignore that which connects them. In fact, one cannot study syntax without the morphology and vice versa, nor can one study the morphology without the phonology and even without the phonetics. The levels, in fact, do not and cannot exist on their own and cannot be studied without taking their interaction into consideration during the very process of study.

We have also in recent times tried to dissect the natural process of development of language by subdividing it into allegedly static synchronic stages. This is the same as if we were to try to subdivide a movement, for example the flight of an arrow, into so many states of rest—a feat which was attempted in vain by Greek philosophers and which has now, I understand, been shown by modern physicists to be impossible. To my mind a synchronic stage may perhaps, although I strongly doubt it, be singled out in the case of a language now spoken but it cannot, unless as a conventional reconstruction, be singled out in the case of a dead language. This is because when we study a dead language we are in actual fact studying a WRITTEN language and the writing always preserves phenomena which date from different periods and includes scribal conventions which are not, and never have been, reflections of any real phenomena in the spoken language.

Then comes still another difficulty. Descriptivists shun the use of what appear to them to be hackneyed terms coined at a particular moment for a language other than the one they are describing and which thus do not express EXACTLY the same thing as they have in view. They fear that, along with such terms, they may bring preconceived notions and false mental associations into their description. To avoid this they coin for each occasion new terms and the number of these has lately grown beyond the point where it is possible to store them in the human brain. And yet in spite of this the descriptivists, just like the neogrammarians before them, continue to drag in preconceived notions along with their new terms, namely those notions which are associated with their own new theories—because a student cannot operate without a theory so long as he continues to think. But the rejection of broad and general terms in favour of terms characteristic of only one language and of only one individual linguistic situation deprives terminology of one of its most important



functions, that of generalization or synthetization. When mental associations are banished the ability for synthesis is also banished and science is atomized and becomes an amorphous mass of incoherent scraps of knowledge. At times the whole achievement of a descriptivist technique is the expression in esoteric terms of some rather trivial truth. A neogrammarian, coming upon the English plural forms *oxen* or *mice* alongside the usual forms in suffixed *-s*, would delve into the history of the language; a descriptivist simply says these forms are allomorphs of the usual plural which, in plain words, simply means that they are unusual ways of expressing the plural—a fact which, of course, we knew all along.

There is at present a very fashionable saying, namely that real science begins with the introduction of figures and formulae. I cannot say I agree with this. To my mind, real science begins when one starts to ask “Why?” and to try to find the answers. Not that I cannot see the importance of rigorous definitions and formalized procedures in linguistics, but it is still more important that we do not lose sight of the main principles of humanistic science, namely: in the first place, to see the object of study as an entity in which all the parts and aspects interact with one another and from which one may not abstract any part from the others without causing a grave distortion of the general picture; and secondly, to see the object of study not as a state or thing, for example an organism, but as an ever changing process, a movement in which all the stages are causally interconnected.

Perhaps my present harangue may seem somewhat out of date, in particular when we consider that descriptive linguistics is rapidly becoming a thing of the past. But the aftermath is still felt, and will continue to be felt for many a year to come—which, by the way, is not quite such a bad thing either, because it is the descriptivists who have made all linguists realize the deficiencies of their traditional techniques. And, to take it from another angle, it is very difficult for a Semitist to be out of date since Semitic studies is apt to preserve antiquated concepts and theories; new trends in linguistics have thus reached the Semitic field before it has mastered the neogrammarian techniques for the reconstruction of *Urformen*.

In any case, if we wish to learn something not of individual Afroasiatic languages but of the Afroasiatic family of languages as a whole—and I presume that this is why experts in the Afroasiatic languages have decided to come together here—then we have to find what it is that these languages have in common and what it is that separates them from all other languages of the world. This cannot be their typology because there is manifestly no simple connection between parallel typological features and genealogical groups of languages; it can only be their genetic relationship the character and the degree of which cannot be discovered otherwise than by a historical approach to the languages in question.

There is nothing impossible in the task. Since, in most branches of Afroasiatic, very little indeed has been done in this direction we need not be fastidious with regard to the techniques to be used. One of my reviewers, Dr. Garbini, has called me a neogrammarian. He is wrong, for my views are very far from the empiricism of

Delbrück or Brugmann and I am very far from rejecting the problems which lie outside the single one of the genetic interrelationship between the languages descending from one particular parent language. On the contrary, my linguistic training has included the search for universals of glottogony and of the stages in the history of linguistic typology. But I do think that genetic relations and reconstructions are in our particular field highly important, perhaps at present the most important, aims of linguistic research. What if someone tried to compile a comparative grammar or a comparative etymological dictionary of, say, the Cushitic languages using the outmoded neogrammarian techniques? I am sure that everybody would be most thankful. It would be an excellent basis for future research in the field of Afroasiatic historical linguistics. If this could be done with the help of some more modern, more rigorous techniques, so much the better; but the trouble is that nothing is being done, whether by means of outmoded or new techniques. Unless I am grossly mistaken very little work has been done in the West comparable to the first ventures into the field of the historical and comparative phonology and vocabulary of Chado-Hamitic languages by the late Moscow scholar Illič-Svityč, or to a similar venture into the Cushitic field by his friend Dolgopol'sky. Marcel Cohen's *Essai comparatif sur le vocabulaire et la phonétique du chamito-sémitique* (1947) was a 'tour de force' and will ever remain an example of selfless enthusiastic labour, a milestone in the history of our science and a work to which several generations of Semito-Hamiticists will return for primary information. But no-one today can fail to recognize that this work, like my own essay in comparative grammar and the similar work by Castellino, has appeared a little before its time. It was an attempt to create a comparative phonology and vocabulary of Semito-Hamitic by jumping the necessary stage of setting up comparative phonological inventories and vocabularies of the individual branches. Strange to say, even for Semitic we still have no equivalent of such works as Walde-Pokorny's historical vocabulary for Indo-European or of Klimov's for South Caucasian. As a result the phonetic reflexes postulated by our dean of Semito-Hamitic studies are more often unconvincing than we would wish and even the composition of the Semito-Hamitic family and of constituting branches must still be considered to be *sub judice*. Many Semito-Hamiticists doubt the possibility of classing Chado-Hamitic along with the Afroasiatic languages—erroneously, as my senior colleague Professor Olderogge has taught me to think—but it would seem that the demonstration of the advisability of such classing has not been convincing enough. Do the High Cushitic and Low Cushitic languages constitute one single branch or two? Would it not be more correct to class High Cushitic with Chado-Hamitic? The answers remain more or less subjective.

A few words regarding the situation in the field of Berber studies, as I see it. Berberists, who are confronted with literally hundreds of very similar and yet differing dialects, have out of necessity long been the only 'Hamiticists' to operate with reconstructed forms, not occasionally but quite systematically. Instead of treating such forms as a neogrammarian would, namely as reconstructed *Urformen* marked



with an asterisk, they call them 'invariants' and regard them as existing at the present synchronic level but manifesting themselves in the various dialects under different phonetic guise. The difference it would seem is more one of philosophical approach than of fact. No one pretends today that *Urformen* with an asterisk represent the identical forms which were once in use in a single parent language. The refusal, however, to look upon the Berber 'invariants' as historical forms—only conventionally historical, of course—leads to the paradoxical fact that, although of all the modern Afroasiatic languages Berber preserves the most ancient forms easily comparable even with proto-Semitic, next to nothing is being done to explain the actual Berber forms from the historical point of view. A case in point is the monumental work by the great Berberist, André Basset, *La langue berbère. Morphologie. Le verbe. —Études des thèmes* (1929). Contrary to what is stated in its title, this book is not at all a study of stems but a non-classified list of stem-types. Anybody starting to learn something about Berber will despair upon finding that this language has something like five hundred verbal types. But, seen from a historical point of view, the picture does not look as black as all that. Should we list all possible Semitic stem types without classifying the stems according to their original phonological composition, that is to say *verba primae aleph*, *verba primae gutturalis*, *verba primae nun*, *verba secundae geminatae*, *verba mediae et tertiae infirmae* according to the good old antiquated terminology, or without grouping the conjugation of these stems by stem-modifications, or *stirpes* (I to XV in Arabic), we would observe the same disheartening picture as in Basset's book on the Berber verb. Classifying Basset's stems into causative (*h*)*a*- and *sə*- prefix stems, reflexive *t*- prefix stems, *m*- and *n*- prefix stems, reduplicated stems, etc., and grouping these stem-modifications according to the consonantal composition of the roots (and, after the work of Rössler and others, I am sure that it might be possible to reconstruct the vanished consonants 'alif, hā', hā', hā', 'ayn, γayn, wāw, yā', each of which has left specific traces in the present day 'invariant' of the verbal root), we will discern a grammatical structure very similar to that of Ancient Semitic and it is a safe prediction that a comparison between the two genetically related structures will be most instructive both for the prehistory of Semitic and for the prehistory of Berber. Unfortunately but little has been done by way of the comparison of these two branches of Afroasiatic, or indeed of any other of the branches with Berber. The very important work of Vycichl and especially of Rössler has been primarily devoted to comparative phonology and, to a limited degree, to the comparison of vocabulary. The few grammatical essays that there are have been partly marred by the absence of any history of linguistic typologies, which has prevented the authors from discerning clearly what is archaic and what is innovatory in the languages under study.

If I might venture to formulate what kind of research I believe is most necessary at the present stage of development of Semito-Hamitic linguistics—in addition to that of continuing with the description of already known and still unknown languages and dialects, a work which for a very long time will be the necessary prerequisite

of all other research—I would, while begging you to keep in mind my status of a virtual outsider, express my ideas in the following terms:

Firstly, one cannot wait until the last button has been sewn onto the uniform of the last soldier before starting a campaign, and likewise, if we want our field of study to be a science and to advance beyond the preliminary amassing of incoherent knowledge, we must add to the task of describing new languages and dialects that of asking “Why?” and of trying to find the answers. The reasons for the existence of any given linguistic phenomenon are rooted in the history of the language and the task to which I am alluding must thus be the creation of a historical and comparative discipline of the Semito-Hamitic languages. The time for this is overdue because we are lagging far behind the students of other fields of linguistics.

Secondly, while the best modern techniques should be applied to our work, even the best techniques will fail to create the needed historical picture if the student lacks a historical approach—while if he does not lack it, even old-fashioned techniques need not in any way be scoffed at.

Thirdly, comparative phonologies, vocabularies and grammars of individual groups and branches of the Semito-Hamitic languages should be commenced as soon as possible; however imperfect these may prove to be at first, without them further progress of our science is unthinkable. The example of the excellent results achieved with modern Iranian dialects spoken in Afghanistan and the Pamir, when studied with an eye to historical reconstruction, or the successful creation of a comparative historical phonology of North-Eastern Caucasian languages which had no writing system of their own up till a few years ago, shows that a history of languages preserved only in their present state such as the Cushitic and the Chado-Hamitic, may and must be reconstructed even if we do not have any Ancient Cushitic or Ancient Chado-Hamitic language to compare with our reconstructions.

Fourthly, a general knowledge of the main features of the languages of the other branches of the family is now indispensable for the achievement of further durable results in the historical reconstruction of any given language or branch of Semito-Hamitic.

And now, lastly, I should say that no one is more aware of the drawbacks of my own little essay in comparative Semito-Hamitic grammar than myself. I believe, and even hope, that the booklet will be completely out of date in a few years' time, and I will be grateful for all criticism for I do not doubt that I have made a number of gross blunders in the presentation of individual languages. But my position as an outsider has allowed me to present a general picture which, it seems to me today, makes some sense. If therefore my work serves to incite new and much deeper studies, delving into the problems of the history of this particular linguistic family which you and I are studying, I shall feel that I have not worked in vain however imperfect my first venture may have been.

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## DISCUSSION\*

BENDER: The situation is not quite as black as Professor Diakonoff thinks with respect to the problems of reconstruction in the Cushitic family. A student from Yale University is going to Ethiopia to start work on the reconstruction of proto-Lowland East Cushitic (essentially Saho-Afar, Galla, Somali, and a few others); Harold Fleming, an anthropologist at Boston University, has attempted a new classification of the whole Cushitic family, which he has now split into Cushitic and Omotic. Furthermore, some of the languages are in better shape now than they were a few years ago: we have outline grammars of Maji, on which there was

\* The text of this and all subsequent *Discussions* is a severely shortened version of the transcript of the tape recording made during the Colloquium. Interventions in foreign languages have been translated into English throughout. (*Editors*).

almost nothing published previously, and of Konso, on which even three years ago Professor Tucker said he had no material; finally, Professor Haberland is at present in Southern Ethiopia collecting material on Ari and Hamar (or Banna). So things are not so bad as Diakonoff thinks.

MARCEL COHEN: In speaking to you just now I forgot to mention Brockelmann among the sceptics. In his review of my *Essai*, while approving the majority of my comparisons, he held to his opinion that the obvious resemblances between the Hamito-Semitic languages were based not upon genealogical unity but on *des affinités*. He did not explain, and no one I think could explain, how these *affinités* could result in so many obvious points of agreement.

LESLAU: There is a slight contradiction I feel in what Diakonoff says: on the one hand he encourages us to reconstruct a proto-Hamito-Semitic and, on the other, he says that he feels his work to be premature. Perhaps I should refer here to Cushitic ... of course we cannot do anything with Ancient Cushitic, we have no documents, but we have many Cushitic languages still spoken. My question to him is: "Does he think that we are in a situation now to reconstruct a proto-Cushitic on the basis of what we have at present as far as the description of the individual languages goes?" My answer is "No!" What we have to do first is to describe the various Cushitic languages (in a very broad sense) still spoken, and only then I think can we go on to either a classification or a reconstruction. I think a classification is extremely dangerous at the present stage. Even in a very small domain of a Semitic Ethiopian language such as Gurage, with a complexity of twelve dialects, a classification is extremely complicated, and the more we know the more complicated it becomes. So I do not think that at the present stage we should try to reconstruct a proto-Cushitic before having the descriptions of the various Cushitic languages.

CARNOCHAN: In the West African area progress is also being made in so far as the Chad languages are concerned. Following up their important article, Newman and Ma are at present in Nigeria with a team investigating the position further, particularly from the point of view of comparative work. As regards classification, Carl Hoffmann says that AT PRESENT he sees no reason why Margi should be included in the Afroasiatic language family at all, while in a recent article Mr. Parsons has questioned whether Hausa is really a Chadic language. Thus the classification of two of the languages about which we have fullest information is being questioned—a situation which perhaps does not arise in the case of languages of which we only know ten words!

MARCEL COHEN: In the forthcoming re-edition of *Les langues du monde*, which will be entitled *Les langues dans le monde*, the chapter on Hamito-Semitic is being written by David Cohen. As I have already written to Diakonoff, we still do not

accept a Hamito-Semitic family with five branches, Chadic being the fifth branch. Chadic will be in a separate chapter in the neighbourhood of Hamito-Semitic but not within it. Meroitic will be excluded from the chapter on Cushitic, being no longer considered to be Hamito-Semitic. Regarding the dangers which prey upon comparatists who neglect the classical rules laid down by Descartes for scientific research, I would quote the example of the late Mlle. Homburger who, on the basis of isolated data 'fished' from various languages, rashly inferred overall relationships. Thus, after attributing an Egyptian origin to all the Negro-African languages, she found common features with Dravidian and spent her last years working towards an Afro-Indian *ensemble*.

ANDRZEJEWSKI: There is only one serious fault with the paper of Diakonoff—he is too diffident about his methods. These are excellent, and there is no substitute for reconstruction. Nothing has arisen in modern linguistics that could be a substitute for the study of regular correspondences. Even the method of mass comparison proposed by Greenberg is justifiable only in situations in the early stages of investigation where reconstruction is impossible. It is only through the study of regular correspondences that we can remove the subjectivity in the matter of assessing what is similar. I differ from Diakonoff in his praise of Dolgopolsky's work on the Cushitic languages—although an excellent *essai*, he departs too far from this basic rule that you cannot really make a valid comparison unless you have a regular system of correspondences, unless you discover the regular patterns of similarity and dissimilarity. Some authors in both the Cushitic and the Semito-Hamitic fields are extremely generous with the concept of similarity.

JUNGRAITHMAYR: I would like to thank Professor Diakonoff in the first place for stressing the historical aspect of our work. Secondly, I wonder whether we could not understand Diakonoff better than it seems that Professor Leslau has done, by seeing his two points in the right order. He said that a Hamito-Semitology should be created and, secondly, that the basis for any further reconstructional work within branches must be the description of the individual languages. I would rather understand him as meaning that he sees the description of the languages, backed of course as he stresses by a historical concept, as a *conditio sine qua non* and that on this basis be set up branch comparative work. I would draw attention to the work being started on the Chado-Hamitic, or Chadic, group of languages—at the Eighth West African Languages Congress held in Abidjan last year a Chadic working group was established and its first Newsletter has just appeared. In connection with Newman and Ma's work, a comparative Chadic word catalogue has been started at the University of Marburg. At least this is a beginning.

GARBINI: Some years ago I wrote a review of Diakonoff's book in which I was too sharply critical of him, but now that I have begun to work in the field of comparative

Hamito-Semitic, taking into account not only the Semitic but also (within the limits of my means) the other members of the family, I have come to see that his work is very valuable. I have written to him to this effect recently, but I now would like to tell him that in my opinion we cannot reach a proto-Hamito-Semitic, because the more we study the ancient states of these languages the more we see—or, better, *I see*—that we do not achieve a reduction in the linguistic phenomena but rather that the field broadens—that is to say that when we go to the more ancient *états de langue* we do not find fewer morphemes and fewer forms but rather more differentiated forms. In the Akkadian period we also have to take into account other language families which do not belong to the Hamito-Semitic group. For example, if we think of root patterns or nominal formations we see that Hamito-Semitic roots divide themselves into tri-consonantal patterns, suffix patterns and prefix patterns. In their historical development we see that the Semitic, Libyco-Berber and Egyptian languages have developed especially the tri-consonantal pattern, whereas in Coptic and Cushitic the suffix pattern has been enriched. The Indo-European languages on the other hand have developed the suffix pattern, but I would like to remind you that in the South Caucasian languages, especially in Georgian, there is a very developed system of prefix patterns and I would ask my colleagues how they explain the fact that in these languages we find the same prefixes as in Semitic, the same morphemes with the same meanings? So I think that our aim must be not to try to find starred forms but to find the different linguistic systems.

ULLENDORFF: A very interesting, stimulating and important paper. In the middle part in particular there was a lot of very good horse sense and I think most of us would feel agreement, and at times even passionate agreement, with some of the things Diakonoff has said. There are, however, other elements in his paper and indeed in his published writings—all of these, of course, show the customary high standard of all his contributions to Semitic linguistics—but probably due to circumstances which are beyond his control he does not seem to have had access to many of the works already published in at least Western countries and his survey as it stands does less than justice to the prevailing state of affairs. For instance, there are such works as Thacker's comparison of the Semitic and Egyptian verbal systems, there is a long string of writings by Polotsky on Semitics and Hamitic languages and, of course, in Egyptology in particular, a very long string of articles which most of us are very familiar with by Professor Leslau on Semitics, Ethiopian languages in particular and their Cushitic substrata, and I myself have sinned in this field many years ago by writing a comparative phonology of the Semitic languages of Ethiopia with particular reference to their Cushitic substrata; we have all I think seen the magisterial writings by Professor Garbini and Professor Fronzaroli which make very important contributions to these comparative historical elements ... they were not referred to, and there are of course writings, which may not be available to Diakonoff, written in Hebrew by such people as Rabin, Kutscher, Goschen, by Barr

(though not written in Hebrew necessarily) which have a direct bearing on these problems. I am reminded of an article which Professor Marcel Cohen wrote *à propos* of something written by Professor Rössler in which he referred to a “bien fâcheuse rupture de la chaîne bibliographique” and I think this is very true. Many of the things which have recently been contributed—and most of these are extremely welcome contributions to the field of Hamito-Semitic—seem to be curiously innocent of what has already been written in this field, and a great deal HAS been done and I think it is worth while to insist on the necessity for people before beginning an article or book to sit down and look at what has already been done. I do not say that they should agree with it, they can discard it, but I think it is only right that they should be aware of what has already been done. Another point, both in Professor Diakonoff’s paper and in a general trend which can be observed in many journals nowadays, particularly in journals where the usual critical criteria of acceptance by stringent editors are not necessarily observed, is in particular that one can observe an obsession with classification. Now classification is a very honorable thing to do, but obsession with classification at the expense of looking at the facts first — many languages, in particular in my field, are not yet in a fit state to be subject to such comparative and classificatory statements. It is also not enough to look at certain vocabulary entries, compare them by statistical studies and reach certain conclusions—in many ways the morphological material is much more important or at least deserves very serious consideration, which in most cases it has not received. I would like to make a plea at the outset to return to perhaps somewhat old fashioned standards both of documentation and of presentation.





## II

### SEMITIC SECTION



## ON THE COMMON SEMITIC LEXICON AND ITS ECOLOGICAL AND CULTURAL BACKGROUND

P. FRONZAROLI

0. It is well-known that while there are several treatises and manuals on the comparative phonology and morphology of the Semitic languages, there is no comprehensive work on the common lexicon. There has been no sequel to the work of Guidi (1878-1879), so faithful to the historical method, or the list of common Semitic words which Bergsträsser (1928) added as an appendix to his *Einführung*.

To answer the second and third questions posed in the First Circular, I intend to use the results of my research on the common lexicon (Fronzaroli 1964a ff.), which for several years I have been carrying out according to a systematic plan. I shall not, however, make any observations on methodology, a subject I have already discussed elsewhere (Fronzaroli 1964a, 1973).

1. In order to give a quantitative answer to the question: "What can be reconstructed of the ... lexicon of the proto-language?", it is first necessary to lay down some principle of selection, in addition to the formal and semantic criteria which enable us to establish the existence of a common word. While the presence of a word in most of the Semitic languages is a good indication that it belongs to the common lexicon, and its presence in two languages may be considered positive evidence of its Semitic character (for example Greenberg 1950, 168), the fact that it is attested in only one language or dialect is not in itself a proof of the contrary. The problem has been discussed, in its relation to etymological dictionaries, by M. Cohen (1947, 52 ff.), who is in favour of including words attested in only one language, providing that they are not obvious borrowings or innovations. At the opposite extreme we have the list drawn up by Bergsträsser (1928), who includes only words attested in all the five main groups (Akkadian, Hebrew, Aramaic, South Arabian-Ethiopic and North Arabic).

Here, however, I intend to confine myself to the study of the lexicon within the Semitic area, with the aim of picking out its most striking characteristics; it will therefore be sufficient to consider as a basis the words that are attested in at least one language or dialect from each of the three main geographical Semitic areas, namely, East, North-West and South-West. It has been necessary, of course, to bear

in mind the various criteria that may be summoned to establish the semantic validity of single comparisons; and I have also had to consider the difficulty of excluding all the words that may have been borrowed. Nevertheless, with these factors in mind, I have carried out my preliminary researches on the principle mentioned above, and have singled out more than 500 lexemes (verbal stems and primary nouns) which conform to the stated requirements. The exact number can be ascertained only when the lexemes have been studied individually. In this calculation, I have considered only once those words that may have had autonomous existence during the common Semitic stage, but which are formally derived from a word already in the list. For example, ŠAYIB- 'white-haired' and ŠAYB-AT- 'white hair', or ŠILY-AT- 'placenta' and ŠALĪL- 'embryo' are counted as single words, like the more obvious instances of the type -WLID- 'to give birth', WALD- 'progeny'.

By way of comparison, we may observe that when Bergsträsser (1928) compiled his list of words attested in the five main groups, he had singled out 156 words, to which he added the numerals and some prepositions and conjunctions. It is also interesting to note the experiment carried out by D. Cohen (1961), who tried to establish "de façon concrète et empirique" the basic Semitic vocabulary, by using the statistical criterion of a wide attestation, and the chronological criterion of meaning persistence. By this means he isolated 306 words or roots, to which should be added the numerals and independent personal pronouns. It is not, however, possible to draw a comparison between our research and the lists compiled by M. Cohen (1947), Greenberg (1966) and Diakonoff (1965), who concern themselves with the Hamito-Semitic field.

1.1. Of all the words in the list, those that have so far been studied separately belong to five associative fields: anatomy and physiology (Fronzaroli 1964b), natural phenomena (Fronzaroli 1965a), religion (Fronzaroli 1965b), wild nature (Fronzaroli 1968) and domestic nature (Fronzaroli 1969). As a result of these researches I have discovered no less than 85 lexemes concerning human anatomy and physiology: these range from general terms for 'man', 'male', 'woman', to ones relating to birth, death and age, bodily functions and illnesses, and parts of the body (some of these can apply to animals as well as man). 31 lexemes concern natural phenomena, referring to parts of the universe, climate and divisions of time. Of the 25 lexemes relating to the sphere of religion, a certain number, while being common terms, have a religious significance only in one part of the Semitic area; to these may be added a few terms connected with the orientation of acts of worship. As regards wild nature, 12 lexemes relate to physical environment, 17 to vegetation, 42 to the animal population; others are attested in only one part of the area, although we may suppose that some of them were common to the whole Semitic area. In the field of domestic nature, there are 31 lexemes which relate to agricultural operations associated with horticulture and plough cultivation and to cultivated plants, and 27 which relate to the rearing of livestock and the species of animals reared.

These figures show that the five associative fields which have so far been studied account for more than half of the lexemes in the list. Let us place them as follows, in order of quantitative importance :

anatomy and physiology	85
wild nature	71
domestic nature	58
natural phenomena	31
the field of religion	30

Four more associative fields have yet to be examined, namely, social organization, working methods, feeding habits and economy, apart from mental processes and general terms, which are not directly relevant to the state of ecological and cultural conditions. If it is borne in mind that the figures quoted refer to lexemes that are attested in all three of the main Semitic areas, then it may legitimately be assumed that the material which the comparison of lexicons has enabled us to isolate will provide a basis for researches into the ecological and cultural conditions of those speaking the proto-language.

With regard to the quantitative distribution into groups according to meaning, although we have laid down different criteria, and set ourselves different aims, nevertheless the data we have collected to a large extent tally with those resulting from the experiment carried out by D. Cohen (1961, 62 ff.).

2. What can be deduced about the ecological and cultural background of its speakers from an examination of the proto-lexicon?

In order to answer this question, it is first necessary to consider the objections raised by the ethnologist J. Henninger (1968) about the importance of borrowings. Henninger, in fact, maintains that the demonstrative force of lexical material is to some degree weakened by the possibility that *Kulturwörter* may have been adopted as borrowings, passing from one language to another, and that the process may even have involved non-Semitic languages. However, as H. Hoijer (1956) has argued very forcibly in a different context, the distinction between the more stable basic vocabulary of a language and its cultural vocabulary, more subject to influence from other languages, is largely illusory. The problem of cultural influences, however, affects the whole lexicon, and must certainly be borne in mind. As far as the Semitic languages are concerned, this subject has been studied for some time. We have, for example, the classic works by Zimmern (1917) on Akkadian borrowings, by Kautzsch (1902) and, more recently, Wagner (1966) on Aramaisms in the Old Testament, by Fraenkel (1886) on Aramaisms in Arabic, and Feghali (1918) on Syriac borrowings in Lebanese Arabic. Regarding the influence of non-Semitic languages, there is the work by Leander (1903) on Sumerian borrowings in Akkadian. Other works relating to this subject are those by Vollers (1896) on borrowings in Egyptian

Arabic, Leslau (1948) on the influence of Cushitic on the Semitic languages of Ethiopia, and Salonen (1952) on substratum words and other borrowings in Arabic. Although all these works are to some extent coloured by the personal opinions of the writers, nevertheless one can reasonably assume that it is possible to isolate borrowed words in our consideration of the common heritage.

Another objection that may be raised at the beginning of this inquiry is that the reconstructed elements are of varying antiquity, and therefore can make no valid contribution to a reconstruction of a synchronic cultural system. But it is the etymology of individual words, that is, the moment in which they were formed, that is of varying antiquity. Some of them, in fact, are of Hamito-Semitic origin, others of Semitic origin, others come from the Sumerian region, or the 'Mediterranean'; while yet others were already regional or dialectal during the common Semitic stage. In our reconstruction of the common lexicon, which we suppose to have been in a synchronic phase shortly before the historical attestations, what we propose to do is to check the existence of the form and meaning of every single lexeme belonging to the period. Then, in order to assess the relevance of the lexicon to the cultural background, it will be necessary to draw our evidence from lexical and associative fields, not from isolated words, as I have demonstrated elsewhere with numerous examples (Fronzaroli 1973). The meaning indicated by the etymology may be significant in those cases where the lexeme is likely to have been formed within the Semitic community as defined above; but it cannot be used indiscriminately.

If we confine our inquiry to the associative fields already mentioned, it is possible to obtain certain indications about the natural environment, the animal population, the vegetation, and the types of economy practised by those speaking the common lexicon. From these indications one cannot expect to deduce a geographical location, but simply some information about the physical environment.

2.1. In the common lexicon the term for a place that is not inhabited, or inhabited only at intervals, is ŠADW- 'steppe', or 'open country'; in some languages the word suggests wildness. There does not, however, appear to be a common term to indicate the desert. It is equally interesting to note that indications that 'open country' or 'steppe' was equated with 'high ground' (which one would expect if the speakers lived in a large valley) are to be found only in innovations peculiar to Akkadian, in which *šadū* denotes 'mountain' and *šēru* (etymologically 'high ground') denotes 'countryside'. Besides well-established terms like NAḤL- 'valley', WABL- 'stream', and NAHR- 'river', the common lexicon also contains a word for 'sea', TIHĀM-(AT-), which may be set against a word, YAMM-, that appears less often in Semitic, but is of greater antiquity, being of Hamito-Semitic origin.

2.1.1. Regarding the absence of a common word for 'mountain', commented on by Guidi (1878-1879), it must be noted that the weakness of arguments *ex silentio*, already obvious (Nöldeke 1899, 14), is also shown by the observations made above

on the range of attestations required before a word can be considered common. A common term for 'mountain' is not verifiable, as the criteria here laid down enable us to include only those lexemes which are attested in each of the three main Semitic areas; but several words are attested in single languages, including some common to several languages, which could be considered common Semitic according to less rigid criteria. For example, there is the Arabic word *ġabal*, which is related to forms attested in the North-West area and in Old South Arabian. On the other hand, it is clear that in some cases, concepts that are particularly important, or which may be considered from various points of view, have been subjected to repeated attempts to re-define them. We find an example of this in the terminology for the points of the compass, where stimuli of a practical nature and stimuli of a religious nature have continually occasioned new definitions.

As for the concept of 'mountain', words attested in various languages show that it could be considered as 'rocky high ground' (as in Ugaritic and in Aramaic the forms that continue *ṢŪR*-), 'steppe uplands' as opposed to 'cultivated land' (as in the Akkadian *šaḏū*), and so on.

2.2. The Semitic lexicon attests a vegetation of seasonal herbaceous plants, closely observed: *ISB*- 'luxuriant grass', *DAT*'- 'new grass', *ḤAḌR*- 'mature grass'. Among the names of species we may recall *DARDAR*- 'thistle', and for their industrial importance, *AŠL*- 'rush', *QANAY*- '*canna*' and *ḤALP*-(*AT*-) 'a herbaceous plant' for which, as Landsberger has recently observed (1967, 41), no definite botanical equivalent has been found. Here, as in the case of ligneous plants, we must bear in mind that the persistence of ancient words was to a large extent conditioned by the drift to new physical environments.

Turning now to ligneous plants, we find there are common terms for 'tree', parts of the tree and 'forest'. Among the names of particular kinds of tree, those with the most widespread attestation are 'oak' *ALL*-(*ĀN*-), 'terebinth' *BUTM*- (from which derives the word for pistachio), 'boxthorn' *AṬAD*-, and 'willow' *ḤILĀP*-, which, it is interesting to note, is semantically linked with the verb denoting the weaving process. We may also observe the 'poplar of the Euphrates' *ḌARB-AT*-, and the 'juniper' *BURĀT*-, both attested only in the Northern area, and the 'tamarisk' *AṬL*-, attested in the West. With regard to particular developments in Arabic, see 2.4.2.

2.3. Turning to the subject of wild animals, which are widely represented, we will confine ourselves to noting that, besides words for members of the cat family, such as 'lion' and 'leopard', for carnivores, such as 'wolf' and 'hyena', and ruminants, such as 'deer' and 'gazelle', the lexicon also records terms for 'elephant' *PĪL*-, 'bear' *DABY*- (*DABB*- in the Western area), 'aurochs' *RI'M*-, and 'onager' *PAR*'-. It must be observed that the distribution of all these animals was much wider than it is now, and that they were found also in the Syro-Palestinian area. The elephant is recorded

as indigenous to Syria up to a thousand years B.C., and the leopard was still found in Palestine during the first decades of this century.

Of less interest here, for the purposes of this inquiry, is the terminology for birds: the most persistent words are 'IŠPÜR-, which was originally a general term for 'small bird', and 'crow' ĠĀRIB-. As for reptiles, there is no term for 'snake', which was under taboo, but we may record WARN- 'varan', and RAQQ- 'tortoise'. The lexicon also attests various names of insects, a word for 'rat' and one for 'frog', while the names of fish are all regional.

2.4. The lexicon shows that primitive hoe cultivation had already gone out by the common Semitic stage. Plough cultivation and horticulture, on the other hand, were practised side by side. Of these two types of cultivation, the former is more widely and comprehensively attested. This has already been pointed out by Guidi (1878-1879), and, more recently, the subject has been studied by Fronzaroli (1960), Widengren (1960), Aro (1964), Salonen (1968, 29 ff.) and, with abundant documentation, Fronzaroli (1969). The generally known terms for 'field', 'ploughing', 'sowing', 'reaping', 'threshing-floor', 'threshing', 'winnowing' and 'sifting', preserved as they are in almost all cases from Ethiopia to Mesopotamia, leave us in no doubt that the operations relating to plough cultivation were already known during the common Semitic stage. In all these words, the regularity of phonological correspondence, the number of derivatives, and the freedom of their semantic development, completely rule out the possibility of their being borrowed. To the above words we may add ŠUBBUL-(AT-) 'ear (of corn)', TIBN- 'straw', ĤINT-AT- (ĤUNT-AT- in the Eastern area) 'wheat', DUĤN- 'millet', and other words less widely attested.

Although they have to some extent been altered by regional evolution, there are also a fair number of widely attested words relating to horticulture. There is, for example, TARIY- 'irrigated', which associates the idea of prosperity and riches with that of plentiful water. Other words are PALG- 'ditch', MIDR- 'loam', WARQ- 'green vegetable', and the names of some cultivated vegetables, such as TŪM- 'garlic', KARAT- 'leek' (different opinion in Vycichl 1963), QITTU'-(AT-) 'cucumber'. Although fruit-growing was probably practised, we cannot establish this through the lexicon, since there are attestations of the names of only a few fruit-trees, and of terms denoting operations such as 'plucking off' -QṬIP-, and 'beating down' -ĤBIT-, which could simply refer to activities associated with the food-gathering process.

2.4.1. Salonen (1968, 31) has divided common Semitic words relating to agriculture into two groups. In the first he places words which he considers to be of greater antiquity, referring to agriculture without irrigation, carried out by means of the dibbing-stick; while the words in the second group are, he maintains, of more recent origin, and refer to cultivation in which both irrigation and the plough were used. Salonen is of the opinion that the ancient Semites may have been living at that time



in Palestine and on the Phoenician coast, where their villages would have had the same economic structure as that in Jarmo, in Iraqi Kurdistan; he also maintains that irrigation methods developed first in Mesopotamia, and from there spread to other areas. We, however, must oppose this view, for there is no linguistic evidence that the terms connected with plough cultivation and horticulture mentioned above found their way into the Western Semitic languages as Akkadian borrowings. If the principle of plough cultivation spread from Mesopotamia towards the Syro-Palestinian area, it must have happened in an earlier epoch than that reflected in the common Semitic lexicon as we conceive it.

2.4.2. Lexical comparison also demonstrates very clearly the existence of secondary developments in the Arabic lexicon. We must here make a distinction between those secondary developments that are also found in other languages in the Southern area, and those found only in the Arabian area. When the semantic evolution is shared by the other Southern languages, it may be attributed to a drift to a new natural environment, which did not possess certain kinds of plant or animal. This would explain the loss of the word for 'oak', 'ALL-(ĀN-), retained in Arabic only in the derivative 'alla 'spear'. In the second category, as Nöldeke already observed (1899, 13), the evolution is more closely allied to the growth of nomadism, and concerns only Arabic, and, to a certain extent, Aramaic, while South Arabian and Ethiopic are not affected. For example, 'ID-, the generic term for 'tree', has retained its meaning in South Arabian and Ethiopic, while in Arabic it came to denote the thorny trees that predominate in most parts of Arabia (Nöldeke 1910, 145). An analogous example in the field of wild animals is the word for 'heifer', 'ARḤ-, which in Arabic took on the meaning of 'young oryx'.

What is even more significant is the change in meaning of words connected with ploughing and sowing, which occurred in the Aramaic and Arabian areas as a consequence of the spread of nomadism. This new kind of life reduced the importance of agricultural activities, which were subsequently observed on the edge of the desert in areas inhabited by sedentary peoples. Thus words were reintroduced as borrowings, spreading from the Canaanite area in the North (Aramaic *zera'*, Arabic *zara'a* 'he sowed'), and from the South Arabian area in the South (Arabic *ḍara'a* 'he sowed'). The influence of the nomadic way of life is also evident in the evolution of the term for 'reaping', which in Arabic came to mean the cutting of the branches and leaves of trees (Arabic *ʾaḍada*, and also *ḥaḍada*); while nouns like BURR-, which originally meant 'wheat that has been harvested and husked', and QAMḤ- 'flour', had in Arabic become general terms for 'wheat'.

2.5. Turning now to the subject of stock-rearing, the common lexicon contains, besides words for 'dog' and 'pig', a large number of terms to denote 'sheep', 'goats' and 'cattle', but only a few relating to the equine family. In general this terminology persisted over the whole area; for not only was stock-rearing practised alongside

cultivation, but it also played an important part in the nomadic way of life. The fact that terms for cattle as well as for sheep and goats persisted does not necessarily mean that the animals had the same importance. While ŠĀ'-, the term for 'a single head of sheep or goats' is linked to ḌA'N-, which means 'flock', with regard to cattle we find only a term to denote a single beast, 'ALP-. A collective noun, BAQAR-, is attested only in Western Semitic languages, and may have been formed at a later period. This may mean that during the common Semitic stage the term for 'a single head of cattle' was more necessary than the term for 'herd', that is, that cattle were reared in small numbers. In this context we may also mention the term for 'udder', ḌIR'-(AT-), in its connection with milk production, and the verb -GZUZ-, 'to shear', relating to the production of wool.

Coming now to the equine family, it is interesting to note that the word for 'ass', which was originally indigenous to Africa and which must have been introduced into Western Asia shortly before the common Semitic stage, is denoted by widely attested terms, ḤIMĀR- for the male of the species, and 'ATĀN- for the female. The word KALB-, denoting 'dog', is common, and presents no problems; it may be of very ancient origin, belonging to an earlier period than the one now under examination. Finally, 'pig' is denoted by a common word, the form of which can be reconstructed, with some reserve, as ḤAZ(Z)ĪR-.

3. In this article we have brought together some of the indications that the examination of the common Semitic lexicon may bring to light. Conclusions about the area inhabited by the speakers of the proto-language may be drawn only when the linguistic data have been collated with archaeological and prehistoric data about the same period.

What it is important to realize is that the relative unity attributed to the language during the common stage is not, in the case of the Semitic languages, merely a working hypothesis. The earliest attestations indicate that West Semitic was not so differentiated in its first historical manifestations as it was later (Moscati 1960; Gelb 1961, 45), while what evidence we have leads us to believe that East Semitic is the product of a fairly recent breakaway and evolution. In relative chronology, we could place as *terminus post quem* the formation of the case system and of mimation. If forms without case endings are attested in the Old Akkadian and Amorite proper names, as well as among Semitic borrowings in Sumerian (Gelb 1969, 84), the period during which the case system and mimation evolved cannot have been much earlier than our oldest texts. But with the application of case endings and object endings to the verb the two areas began to differentiate, as mimation does not exist in the Akkadian verb.

As for the supposed unity of the linguistic Semitic area, this too must be understood in a historical, not an abstract sense. We must not jump to the conclusion that before the Akkadians broke away, the area inhabited by peoples speaking Semitic languages might not have been to some extent linguistically differentiated (as is

shown by some of the words mentioned above, for example, the pair  $\text{HINT-AT-}$  and  $\text{HUNT-AT-}$ ); nevertheless we may assume that the area constituted a continuum, such as to permit the rapid spread of innovations.

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## DISCUSSION

SAGGS: My points are merely of detail. I agree with and appreciate the paper very much indeed as a whole, but there were two very small points of detail I would like to refer to which affect the general use of the method in detail. The first is the question of the word KALB- as an example. Do we in fact know that KALB- originally did mean specifically the domestic dog? I ask that because in Sumerian the ideogram used for the equivalent of KALB- does seem to mean any large fierce animal. It is used, suitably qualified, for 'lion', 'leopard', 'wolf', and other animals. Therefore I would suggest the possibility that the term KALB- may originally have meant a dog-like animal rather than what we call the domestic dog—which might affect the question of the period at which this word developed. Another point about the material culture: one term adduced is the root -GZUZ- 'to shear'. Now we do know that at least in the Near East—I cannot speak for Egypt—the usual method of wool

production down to about the Nuzi period was normally plucking rather than shearing. This therefore raises the question of when the root -GZUZ- came to mean specifically 'to shear', in the sense of shearing wool. There is also the archaeological problem of when efficient shears were first produced, since of course shearing as a process could not begin without the production of efficient shears. I do not know the archaeological evidence about that but I am not aware of any tool of that type in the Near East until well into the second millennium.

CASTELLINO: About the Sumerian name of the dog: we have at least two different names, one is UR BARA which would be the wolf because it is 'the outside dog', and then we have UR with another sign which is read differently by different scholars. Usually they read now EGI, thus UR EGI, which would be 'the domestic dog'. And then there are other special names with UR. In fact there are many different names for animals with the prefix UR, which stands for the type of animal as the general word.

TYLOCH: I think that the conclusions reached by Dr. Fronzaroli are undoubtedly correct. It is well known that the lexicon reflects the cultural and social life of a people. In our own lexical analysis we were able to trace still more material relating to agriculture. In the light of all this we can safely say that the Semites in their pre-historic period were not concerned only with nomadism—a theory which must be re-examined—but also with agriculture. The lexical items relating to agriculture make up a fair proportion of the basic lexicon. If we say that the basic vocabulary comprises 300 words or, following Professor Cohen, 500 words we find that about 40 out of these deal with agriculture excluding viticulture and horticulture. It would thus appear that agriculture was well known to the Semites at the start of their history and, although the problem regarding the place of their origin is not yet solved, we can say that at the beginning they were not only nomads but comprised also sedentary people who were concerned with agriculture in a broad sense.

FRONZAROLI: I am grateful for all the suggestions that have been made and for what has been said concerning the word for 'dog'. In my contribution I simply said that FROM THE POINT OF VIEW OF SEMITICS the word did not pose any problems, which does not of course imply that there are no problems as regards the material culture. Concerning this item and others I would like to stress that a fuller treatment is to be found in my articles referred to in the bibliography of my paper.



## THE EVIDENCE OF THE PROTO-LEXICON FOR THE CULTURAL BACKGROUND OF THE SEMITIC PEOPLES

WITOLD TYLOCH

Although the problem of the origin of the Semitic peoples has for long worried scholars,<sup>1</sup> none of the answers proposed to date have been convincing enough to receive general acceptance. The location of the original settlements of the Semites is, nevertheless, very important because it would facilitate the determination of their original way of life. While the problem has been lengthily discussed, it has not found any universally approved solution. Up till a short time ago the opinion prevailed that the Semites were originally nomads.<sup>2</sup> There exists in addition, however, a contrary opinion which asserts that the Semites were also concerned with agriculture from the very beginning of their history (Widengren, 1960). Especially interesting and valuable in this respect are the arguments proposed by Fronzaroli (1960) who, basing himself on the findings of prehistoric archaeology, concluded that the original country of the Semites is to be sought in the developed agricultural culture of the territories of Syria and Palestine and that their nomadism was the result of a later evolution.

If the question of the place of origin of the Semites is as yet unresolved, that of the nature of their original culture would seem to us to be reasonably well indicated. We are confirmed in that opinion by the analysis of the common Semitic proto-lexicon, one of the more recent contributions to which is the study by Aro (1964) of the common Semitic agricultural terminology in which he deals more particularly with the 'bread making terms'.<sup>3</sup> The question of the agricultural terminology demands, however, a broader examination and this has in fact already been systematically undertaken by Fronzaroli (1964a, b; 1965a, b; 1968). His method of study is undoubtedly correct, for it is a well known fact that the lexicon reflects the concrete

<sup>1</sup> See especially Guidi (1878-1879); Clay (1919a and 1919b); Monet (1923); Conteneau (1937); Kupper (1957); Edzard (1957); Matouš (1958); Moscati (1960a); Henninger (1968); Salonen (1968).

<sup>2</sup> See Moscati (1958, 1959, and 1960b); de Vaux (1958), 129, "... jusqu'à l'entrée assez tardive des Arabes dans l'histoire, les Sémites nous apparaissent comme les anciens pasteurs de petit bétail qui vivaient dans la zone sub-désertique cernant le désert syro-arabe".

<sup>3</sup> Aro (1964, 475): "Zunächst können wir feststellen, dass wir eine ziemlich vollständige Reihe von Ausdrücken finden können, die sich auf den Werdegang des Brots beziehen".

life situations of a people or of a tribe. In an analysis of those roots which are generally accepted as being proto-Semitic, we may assume that among them there will be terms which on the basis of their etymology testify as to the ecological background of the Semitic peoples. It is evident from such an analysis that the Semites were as well acquainted with agriculture as with animal husbandry.<sup>4</sup>

The following list of roots relating to the ecological background belong, on the basis of their etymology, without question to the Semitic proto-lexicon. Almost all of them are represented in each of the basic Semitic linguistic groups.

- (1) *'dm* NWS (North-West Semitic): Hebr. *'ādhāmā* 'arable soil with water and plants'; Aram. *'ādhm<sup>e</sup>thā* 'arable soil'. SWS (South-West Semitic): Arab. *'i'dāmat* 'soil without stones'.
- (2) *'ḥd*: NES (North-East Semitic): *'aḥāzu* 'to seize, grasp'. NWS: Ug. *'ḥd* 'to seize'; Hebr. *'āḥuzzā* 'landed property'; Aram. *'āḥūztā* 'landed property', *'aḥīdhūthā* 'lease, tenancy'; SWS: Arab. *'ḥād* 'land occupied by somebody', *'iḥādat* 'landed property'; ESA (Epigraphic South Arabian): *m'ḥd* 'dam'; Ge'ez *ma'aḥaz*, *ma'aḥazat* 'landed property, occupied land'.
- (3) *'kr*: NES Akk. *ikkaru* 'farmer, ploughman'; Old Bab. *ikkarūtu* 'ploughing, to work on the land, to settle down in the country'; NWS: Hebr. *'ikkār* 'farmer'; Aram. *'ikkārū*, Syr. *'akkarā* 'ploughman, tiller', SWS: *'akkār*, *'ikkār* 'farmer, villager'.<sup>5</sup>
- (4) *'lp*: NES: *alpu* 'ox, neat'; NWS: Ug. *'alp*, Phoen. *'lp* 'ox'; Hebr. *'eleph* 'cattle'; Aram. *'lp*, *'lp* 'ox'<sup>6</sup>; SWS: Ge'ez *'əlf* 'thousand'.
- (5) *'/pr*: NES: Old Akk. *ipru* 'field'; Old Bab. *eperu* 'earth, soil, dust'; NWS: Ug. *'pr* 'dust', Canaan. (El Amarna—Knudtzon 1915, 592, 598) *aparu*, *ḥaparu* 'dust'; Hebr. *'ēpher* 'loose soil, dust', *'āphār* 'dry, fine crumbs of earth (dust)'; Aram. *'ephru* 'dust, (pasture) land, meadow', *'aphrā* 'dust, mud, sand'; Syr. *'ūpharā* 'dust, chaff'; SWS: Arab. *'afr*, *'afar* 'dust (of the earth)'; Ge'ez *'afar* 'dust, earth, soil'.
- (6) *'rd*: NES: *erše/itu* 'earth, soil, dry land'; NWS: Ug. *'rš*, Phoen. *'rš*; Hebr. *'ereš*, Aram. *'r'ā*, *'arqā* 'earth, dry land, field'; SWS: ESA *'rd*, Arab. *'arḍ* 'earth, country, soil'.
- (7) *b'/wr*: NES: Akk. *beru* 'well, water place'; NWS: Ug. *b'r*, Phoen. *bwr*, Hebr.

<sup>4</sup> I have utilized here in part the results of an unpublished work by M. I. Grinfeld "Terminologia rolnicza w językach semickich" (The agricultural terminology in the Semitic languages), Department of Semitic Languages of the University of Warsaw, 1963.

<sup>5</sup> Fränkel (1886, 128) considers the Arabic *'akkārūn* to be a borrowing from Aramaic; M. Cohen (1947, 77) connects with this root NWS *hagar* 'soil, country, town', SWS (ESA) *hgr* 'ploughland', Sumerian AGAR and Latin *ager* and regards this term as a 'mot voyageur'. The term is already attested in Old Akkadian, and is borrowed from Sumerian ENGAR.

<sup>6</sup> This meaning is given in Vinnikov 1958, 201. The term also means 'a thousand' in all the Semitic languages,



*bē'ēr*, Aram. *bē'ērā* 'water place, well (of underground water)'; cf. Hebr. *bôr* 'water-pit, cistern'; SWS: Arab. *bīr* 'well'.

(8) *b'r*: NES: Old Akk., Old Ass. *būrū*, Middle Bab. *bī/ēru* 'young bull'; NWS Ug. *b'r* ('to lead'), Hebr. *bē'ir*, Aram. *bē'irā* 'cattle, beasts', Syr. also 'beasts of burden and domestic animals'; SWS: ESA *b'r* 'domestic animal, big livestock, small livestock', Arab. *ba'ir* 'beast of burden, camel, ass', Ge'ez *bə'arāy* 'ox (esp. for ploughing), cow, cattle'.

(9) *bqr*: NES: *buqaru*<sup>7</sup> 'cattle, ox'; NWS: Hebr. *bāqār*, Aram. *baqrā*, *baqrētā* 'herds of cattle, oxen, cows' (as animals used in the span or yoke—for ploughing).<sup>8</sup>

(10) *gmn*: NES: *gannatu*, *gannu* 'garden'; NWS: Ug. *gn*, Phoen. *gn*, Hebr. *gan*, Aram. *ginnā*, *gintā*, '(fencing in) garden'; SWS: ESA *gnt* 'cover', Arab. *ġanat*, Ge'ez *gan(n)at* 'garden'.

(11) *gpn*: NES: Akk. *gapnu* 'shrub'; NWS: Ug. *gpn*, Hebr. *gephen*, Aram. *guphnā* 'vine'; SWS: Arab. *ġafn* 'grape-vine'.

(12) *grn*: NES: Old Akk. *magrattum* 'threshing-floor', *garānu* 'to collect, to pile, to store'; NWS: Ug. *grn*, Hebr. *goren*, Aram. *gōren*, *gūrnā* 'threshing-floor', SWS: Arab. *ġarīn*, (*ġurn*) 'threshing-floor, the place where the dates are put to dry', Ge'ez *g<sup>w</sup>arn*, *gurn*, *gorn* 'threshing-floor'.<sup>9</sup>

(13) *grš*: NES: Akk. *guruššu* 'who is foddering, fattening the animals'; NWS: Ug. *grš* 'to drive out', *ygrš* 'driver (n. of a stick)', Hebr. *mighrāš* 'pasture-ground, fallow land', Aram. (Jew. Pal.) *migrāš* 'Vorstadt, eig. Ort, wohin man das Vieh treibt' (J. Levy); SWS: Arab. *ġašara* 'to send beasts to pasture', *ġašar* 'the livestock in the pasture', *ġušār* 'livestock, herd', Ge'ez *gārša* 'nomen cibi, fortasse offa?' (Dillmann).

(14) *dwš*: NES: Akk. *dāšu* 'to tread on (threshing oxen)', Old Bab. *dā'īšu* 'who is threshing', *dayaštu* 'threshing sledge'; NWS: Ug. *dī(?)*, Hebr. *dāš* (*yādūš*), Aram. *dāš* (*yēdūš*) 'to trample on, thresh'; SWS: Arab. *dāsa* 'to thresh', *dāyis* 'fruits of the earth on the threshing-floor', *midwas* 'threshing-sledge'.

(15) *qrh*: NES: *zarū* 'to winnow' ('the instrument for winnowing' = *rapšu*); NWS: Ug. *dry*, Hebr. *zārā*, Aram. *dērā* 'to winnow' (also 'to scatter'); SWS: Arab. *darāy* 'to winnow', Ge'ez *zarawa* 'to scatter' ('to winnow' = *ša'aya*).

(16) *qr'*: NES: Old Akk. *zēru* 'seed, grain', *zarū* 'to winnow' and 'to sow'; *mdr'* 'the sown', Phoen. *zr'* 'seed', Hebr. *zera'* 'seed, offspring', *mizrā'* 'seedland', Aram. *zērā'ōn* 'seed', Syr. *zar'ā*, *zērā'* 'seed, grain'; SWS: ESA *mqr'* 'seed time, sowing, plant', Arab. *zar'un* 'seed, sowing', Ge'ez *zare'* 'seed, sowing'.

(17) *qwn*: NES: Akk. *šēnu* '(small) livestock'; NWS: Ug. *š'n*, Hebr. *šō'n*, Aram. *'ānā* 'small livestock, herd (sheep and goats)'; SWS: ESA *š'n*, Arab. *qā'n* 'small livestock'.

<sup>7</sup> See Archives royales de Mari, t. XV, Répertoire analytique des tomes I à V; see also Bottéro and Finet (1954), 194.

<sup>8</sup> See Köhler and Baumgartner (1958), 145; see also Widengren (1960), 399.

<sup>9</sup> Fränkel (1886, 25) considers the Arabic form to be a borrowing from Syriac *gūrnā*.

- (18) *hlq/hql*: NES: Old Akk. *eqlu* 'field'; NWS: Phoen. only in *'šmnhlq* (Lidzbarski 1898, 275), Hebr. *heleq* 'share of possession, share of the land, plot of land', Aram. *helqā*, *h<sup>a</sup>laq* 'field, portion, share', *haqlā* 'field', *haqlāyā* 'farmer'; SWS: ESA *hlq* 'field, soil', Arab. *haql*, *maḥqalat* 'sown field, sowing', Ge'ez *haqəl* 'field, soil'.
- (19) *hmr*: NES: Old Akk. *imēru* (*emāru*, *emēru*, *imīru*), Assy. *himāru* 'ass'; NWS: Ug. *hmr*, Hebr. *ḥāmōr*, Aram. *ḥāmārā* (Syr. *h<sup>e</sup>mrā*) 'ass'; SWS: ESA *hmr* 'ass', Arab. *himār* 'ass, onager'.
- (20) *hrt*: NES: Akk. *erēšu* 'to plough' ('Saatpflug gebrauchen'), *erištu* 'sowing, planting' (also *me/irištu*, *mērēšu*, *mērēštu*); NWS: Ug. *hrt*, Hebr. *ḥāraš* 'to plough' (*maḥārēšā*, *maḥārešeth* 'ploughshare'), Aram. (Jew. Pal.) *ḥarišā* 'ploughing' (*maḥārēšā* 'ploughshare'), Syr. *h<sup>e</sup>rath* 'to incise (plough)', *hūrāthā*, *meḥarthanūthā* 'ploughing, agriculture, ploughland';<sup>10</sup> SWS: ESA *mhr̥t* 'ploughland', Arab. *ḥaraṭa* 'to plough' (*miḥraṭ* 'ploughshare'), Ge'ez *ḥarasa* 'to plough' (*māhras* 'ploughshare').
- (21) *h/hzr*: NES: Akk. *huzirum*, *huzirtu* '(wild) swine'; NWS: Ug. *hznr*, Hebr. *ḥāzīr*, Aram. *ḥāzīrā* '(wild) swine'; SWS: Arab. *ḥinzīr*, Ge'ez *hanzīr* 'swine'.
- (22) *kpr*: NES: Akk. *kapru* 'village'; NWS: Ug. *kpr*, Hebr. *kopher* (*kāphār*), Aram. *kāphrā*, *k<sup>e</sup>phār* 'village'; SWS: ESA *kfr* 'village, place', Arab. *kafr* (*qafr*) 'village'.
- (23) *krm*: NES: Akk. *karmu* 'vineyard', *karānu*, *karnu* 'vine';<sup>11</sup> NWS: Ug. *krm*, Hebr. *kerem*, Aram. *karmā* 'vineyard'; SWS: Arab. *karm* 'vine, grape-vine', Ge'ez *k<sup>e</sup>rm*, *karm* 'vine'.
- (24) *ngl*: NES: Akk. *nigallu*, *nīgālu* 'axe', Old Bab. *aggullu* (*akkullu*) 'sickle, axe'; NWS: Hebr. *maggāl*, Aram. *maggelā* 'sickle'; SWS: Arab. *minḡal* 'sickle, scythe'.
- (25) *nwy*: NES: Akk. *nawū*, *namū* 'pasture, steppe'; NWS: Hebr. *nāwe*, *nāwā* 'pasture, grass-land', Aram. *nāwā* 'separated place'; *nawithā* 'pasture'; SWS: ESA *nwy* 'pasture, meadow, verdure'.<sup>12</sup>
- (26) *nhl*: NES: Akk. *naḥālu* 'to sift, winnow'; NWS: Aram. *neḥal* 'to sift, pass through a sieve' (*meḥultā*, *maḥḥultā* 'sieve'); SWS: Arab. *naḥala* 'to sift' (*munḥul*, *munḥal* 'sieve').
- (27) *gl*: NES: Akk. *agālu* 'mule' (Bab. and Assy. *agalu* 'saddle-ass'); NWS: Ug. *gl*, Phoen. *gl*, Hebr. *ēghel*, Aram. *eglā*, *iglā* 'young bull, calf, young male neat'; SWS: Arab. *īḡl*, *īḡḡawl* 'calf', Ge'ez *'egwāl* 'young animal', *'egwalt* 'heifer'.<sup>13</sup>
- (28) *'zq*: NWS: Hebr. *'aziqa* 'hoed ground', Aram. *'azēqā* 'field ploughed for the first time'; SWS: Arab. *'azaqa* 'to hoe'.
- (29) *'ḏd/hṣd*: NES: Akk., Bab., Assy. *eṣēdu*, *eṣīdu* 'to harvest, reap'; NWS: Aram. *ḥeṣad* 'to harvest', Hebr. *ma'aṣādh* 'crooked billhook for wood-cutting';

<sup>10</sup> Aro (1964, 475) points to the fact that Aramaic has another form *dēbar padduna* 'to conduct a plough' and that *k<sup>e</sup>rab*, Arab. *karaba* 'to plough' is not borrowed from the Akkadian (*ibid.*, No. 1).

<sup>11</sup> M. Cohen (1947, 115) gives two terms: 184 *karānu* 'wine' and 186 *krm* 'vineyard, vine' but connects the two roots.

<sup>12</sup> In Punic we find a proper noun *b'l mgl'* (Lidzbarski, 1898, 240). Arabic *nawā* has another meaning: 'to drive, keep, wander from place to place; to become fat (of a camel)'.

<sup>13</sup> In ESA we find only the proper noun *gl* - *'āgil* (Conti Rossini 1931, 201).

SWS: ESA *h̄zd* ‘harvest month(?)’, Arab. *ḥaṣada* ‘to harvest’ (*miḥṣad* ‘sickle, bill-hook’), ‘*aḍada* ‘to cut off’, Ge‘ez ‘*aḍada* ‘to harvest’ (*mā‘eḍad* ‘sickle, scythe, crooked knife’).

(30) *plg*: NES: Akk. *palgu* ‘canal’; NWS: Phoen. *plg* ‘region’, Hebr. *pelegh* ‘canal’ (Aram. *pēlagh*, *palgā*, *pulgā* ‘part, section, division’); SWS: Arab. *falğ*, ‘*aflağ* ‘brook’, Ge‘ez *falag* ‘stream, canal, valley’.

(31) *šmd*: NES: Old Akk. *šimdu*, Bab. *šamadāni* ‘span’, *šamadu* ‘to bind together, harness’; NWS: Ug. *šmd* ‘rod, yoke’, Phoen. (*s*)*smdt* ‘span of oxen’, Hebr. *šemedh* ‘span’ (also: ‘measure of land which a span can plough in a day’), Aram. *šimdā* (*šemedh*) ‘yoke (for oxen)’, *šāmidh* ‘pair joined by a yoke’; SWS: ESA *ḍmd* ‘to couple, to join’, Arab. *ḍamada* ‘to join, bind’, Ge‘ez *ḍemd* ‘yoke, pair, cart harnessed with a pair of animals’, *maḍmad* ‘bridle, cord’.

(32) *qšr*: NES: *kašāru* ‘to bind, collect’; NWS: Hebr. *qāšar* ‘to harvest, reap, reap the harvest’, Aram. (Syr.) *qṯr* ‘to bind’; SWS: Ge‘ez *qēšara* ‘to bind’.

(33) *rhṯ*: NES: Akk. *rāṯu* ‘watering trough (for cattle)’; NWS: Heb. *rahaṯ* ‘watering trough’, Aram. *rāheṯā* ‘channel bringing water to a trough’.

(34) *rṯb*: NES: Akk. *raṯābu* ‘to moisten, humidify’; NWS: Hebr. *rāṯabh* ‘to be moist’, Aram. *rēṯbā* ‘wet, moist ground, humidity, verdure’; SWS: Arab. *raṯaba* (*raṯiba*) ‘to be fresh, moist’, Ge‘ez *raṯba* ‘to be moist, succulent, fresh’.

(35) *r’y*: NES: Akk. *rē’ū* ‘to pasture, tend’, *ru’u* ‘shepherd’, *ri’itu*, *ritu*, *rittu* ‘pasture, herd’, *merētu* ‘pasture’; NWS: Ug. *r’y* ‘shepherd’, Phoen. *r’* ‘shepherd’, Hebr. *rē’ī* ‘meadow, pasture’ (also *mir’e*, *mar’ith*); Aram. *ri’yā* (*mir’ā*) ‘pasture’, *rā’ayā* ‘shepherd’; SWS: ESA *mr’y* ‘pasture’, Arab. *ra’ay* ‘to tend, graze’, Ge‘ez *rē’ēy* ‘to tend’ (*mar’et* ‘livestock driven to pasture’).

(36) *šdw/y*: NES: Akk. *šadū* ‘mountain’; NWS: Ug. *šd*, Phoen. *šd*, Canaan. (El Amarna) *šatē*, Hebr. *šādhe*, *šaday*, Aram. (Jew. Pal.) *šādhe* ‘field, acre, country, open field’; SWS: ESA *šdw* ‘field’, Arab. *sadin* ‘moist soil fitting for cultivation’.

(37) *šbl*: NES: Akk. *šubultu* ‘ear (of grain)’; NWS: Ug. *šblt*, Hebr. *šibboleth*, Aram. (Jew. Pal.) *šubaltā*, Syr. *šebhletā* ‘ear (of corn)’; SWS: Arab. *sunbulah* ‘ear (of corn)’, Ge‘ez *sabl* ‘ear (of corn)’ (*sanbīl*, *sanbal* ‘spica aromatica’).

(38) *šqy*: NES: Akk. *šaḳū* ‘to give to drink’, *šik/gītu* ‘irrigation’, *mašqu* ‘place for watering cattle’, *mašqitu* ‘installation for irrigation’; NWS: Ug. *šqy* ‘to drink’ (or ‘to give to drink’), Hebr. *šoqeth* ‘watering trough (for cattle)’, Aram. *šaqyā*, *šaqethē*, (Syr. *šeḳyā*, *šaqayā*) ‘irrigation’; SWS: ESA *sqy* ‘irrigated field’, *sqyt*, *msqt* ‘canal’, Arab. *sāqiyat* ‘canal (for field irrigation)’, *suqyā* ‘irrigation’, Ge‘ez *saqāyi* ‘who irrigates’, *sēqyat* ‘irrigation’.

(39) *tīm*: NWS: Ug. *tīm*, Hebr. *telem*, Aram. *talmā*, *telāmā* ‘ridge, furrow’; SWS: Arab. *talam* (*tilīm*) ‘ridge’; Ge‘ez *tēlm* ‘ridge, furrow’.

(40) *ṭwr*: NES: Akk. *šūru* ‘bull’; NWS: Ug. *šr*, Hebr. *šōr*, Aram. (Jew. Pal.) *tōrā*, Syr. *tawrā* ‘bull’; SWS: ESA *ṭwr*, Arab. *ṭawr*, Ge‘ez *sor* (and *sawr*) ‘bull’.

The terms quoted above belong, as can be shown on the basis of other evidence, to the Semitic proto-lexicon. A small number of them (for example *’kr*, *grš*, *kpr*)

were borrowed from other languages, but all entered Common Semitic at a very early stage in its development. Taken together these terms, whether native or borrowed, prove that agriculture was known to the Semitic peoples from the earliest times. It would therefore appear that the widely held views concerning primitive Semitic nomadism should be reconsidered. The terms dealt with concern only the rearing of domestic animals and the cultivation of land, activities which are typical of a settled agricultural people. This conclusion would seem to be even more justified were we to take into account also those terms which deal with horticulture and viticulture.

The analysis of the agricultural terms of the proto-lexicon appears, therefore, to confirm the statement by Fronzaroli that the Semites in their earliest period were at least in part a sedentary people to whom agriculture was well known (Fronzaroli, 1960). If we accept that the basic lexicon comprises somewhere between three and five hundred words,<sup>14</sup> the agricultural terms constitute a considerable and important part of it and these terms are concerned with sedentary agriculture and not simply with a nomadic way of life.

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This paper was not discussed, having been received too late for advance distribution.



## BEITRÄGE ZUR HAMITO-SEMITISCHEN WORTVERGLEICHUNG

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Bei dem Versuch, die am letzten bekanntgewordene Gruppe der 'großen semitischen' (hamito-semitischen oder afro-asiatischen) Sprachfamilie, nämlich das sogenannte Tschadohamitische, durch Nachweis von Gemeinsamkeiten in Grammatik und Lexikon noch stärker mit den übrigen Zweigen dieses Sprachstammes zu verbinden, kann es sich nur um vereinzelte Nachlesen handeln. Hinsichtlich der vokabularischen Parallelen wurde schon von Anfang an betont (Lukas 1936a, 587), daß das Ergebnis beschränkt sein wird, da die tschadohamitischen Sprachen in ihrem Wortschatz sehr stark von Nachbarsprachen beeinflußt worden seien. Trotzdem haben komparatistische Untersuchungen (Cohen 1947; Greenberg 1966; Illič-Svityč 1966; Hodge 1968; Rössler 1964) eine ganze Reihe von Wortgleichungen zutage gebracht, und man darf wohl annehmen, daß die 'klassischen Schlüsselwörter' (Jungraithmayr 1964, 163), die das Tschadohamitische mit den übrigen hamito-semitischen Sprachen gemeinsam hat (z.B. die Wörter für 'Mund', 'Zunge', 'Zahn', 'Wasser', 'Rind', 'Name', 'sterben', 'essen', 'bauen'), bereits sämtlich entdeckt wurden. Der folgende Beitrag soll, ohne längst Gesagtes noch einmal zu wiederholen, eine Reihe weiterer Etymologien zur Diskussion stellen, wobei nicht nur Neues gebracht wird, sondern auch bereits vorgeschlagene Wortgleichungen nach der einen oder anderen Seite ergänzt wurden. Von den semitischen Sprachen im engeren Sinn des Wortes wurden besonders das Südarabische mit seinem altertümlichen Wortschatz und die Semitensprachen Abessiniens herangezogen. Da sich der Verfasser für die übrigen Sprachzweige nicht kompetent fühlt, ist er — vor allem für das Tschadohamitische — für Kritik von Seiten der Afrikanisten dankbar. Er hofft, auch nicht der Versuchung erlegen zu sein, mögliche arabische Lehnwörter als urverwandt ausgegeben zu haben. So wurden, obwohl in den Glossaren nicht als solche vermerkt, Wörter wie jegu *fas* 'Axt', log. *zibir* 'Spanne', *sámàn* 'fett sein', mubi *nuggara* 'Trommel' (aus dem auch in europäischen Sprachen entlehnten arab. *naqqāra*, wozu wohl auch das seinerseits in kuschitische Sprachen übernommene als 'Sprecherin' gedeutete amh. *nāgarit* gehört), mus. *buhūm* 'Rind', *sūnsuli* 'Kette' (das man freilich eher von äth. *sānsāl* als von arab. *silsila* herleiten möchte), sok. *gēru* 'Nachbar', sura *bish* 'schlecht, böse', *bār* 'genesen' und zahlreiche andere unter dem Verdacht, arabisches Lehngut zu sein,

außer acht gelassen. Das untersuchte Vokabular gehört, wovon man sich leicht überzeugen kann, fast ausschließlich dem Grundwortschatz der Sprache an. Durch Übernahme aus verschiedenen Quellen bedingte Inkonsistenzen in der Umschrift möge man bitte entschuldigen.

- (1) sem. (<kusch.) har. *zway āy* 'Mutter'; kusch. wol. *ayē* 'Mutter'; ga. *ayo* (Leslau 1963, 38); som. *hooyo*; tschad. kot. *iya, iya, ya* 'Mutter' (Sölken 1967, Nr. 540); mubi *iyà* (Lukas 1937, 182). Das Wort ist allerdings in Afrika auch sonst verbreitet, vgl. gulfan *aya*, midob *iya*, suk *iyu*, nandi *eiyo* (Greenberg 1966, 102) und daza *aya*, fur *iya*, gumuz *iyō* (ebd. 142).
- (2) sem. *b'l* 'Herr' (Bergsträsser 1928, 182); tschad. log. *bəl-e, bile* 'Mensch, Mann' (in Genitivverbindungen), z.B. *bile yaā* 'Hausherr', *bile ngāma* 'Rinderhirt' (Lukas 1936b, 86).
- (3) sem. tē. *gāmār* 'Vulva' (Reimwort zu *šāmār* 'Penis?'); meh. *gīber*; tschad. kot. *gerīm, girim* 'Frau, Vulva' (Sölken 1967, Nr. 225); log. *ganām* 'Frau' (Lukas 1936b, 93); bud. *ngirim* (Lukas 1939, 122). Mukarovsky (1969, 343) erschließt aus kusch. moča *gānne* 'Dame' — wozu noch kafa *genē* 'alte Frau' und agau *ganā* 'Mutter' gehören, deren frappierende Übereinstimmung mit indogerm. *\*gʷenā* 'Frau' natürlich nur zufällig sein kann — und den Wörtern im Logone und Buduma ein *\*gérīm*.
- (4) sem. meh. *šfit* 'Haar'; šaḥ. *šfēt*; soq. *ša'f, šfeh*; arab. *ša'afa* 'Haarlocke'; tschad. sura *shwəp* 'Haar' (Jungraithmayr 1963, 83); mon. *súwəp* (Jungraithmayr 1964, 179).
- (5) sem. *'dn* 'Ohr' (Bergsträsser 1928, 183); arab. *'uḏn*. Für eine mögliche ägyptische Entsprechung s. Cohen (1947), Nr. 16, Diakonoff (1965), 52; tschad. jegu *'údūḡē*, pl. *'ūdāḡ* 'Ohr' (Jungraithmayr 1961, 117); kar. *dēḡgei* (Lukas 1937, 17).
- (6) sem. *\*šafat* 'Lippe' (Nöldeke 1910, 127ff.); hebr. *šāfāh*; arab. *šafa*; äg. *šp.t* 'Lippe' (Cohen 1947, Nr. 287); tschad. sura *mishpwə* 'Lippe' (Jungraithmayr 1963, 75); ha. *lebe* (Diakonoff 1965, 42 mit Fragezeichen) gehört kaum hierher.
- (7) kusch. bad. *qinō, qinē* 'Kopf' (Cerulli 1951, 485); tschad. *\*k-(n)* 'Kopf' (Newman-Ma 1966, Nr. 47); vgl. jedoch Greenberg (1966, Nr. 36).
- (8) sem. arab. *qafā', qafan* 'Hinterkopf, Nacken'; berb. *eqəf* 'Kopf'; tschad. jegu *kofo* 'Hinterkopf, Gehirn' (Jungraithmayr 1961, 114).
- (9) sem. akk. *pānu* 'Angesicht'; hebr. *pānāh*; äth. *fāna* 'in Richtung nach'; meh. *fenē* 'vor'; šaḥ. *fené*; soq. *fēne*; kusch. agau *feni* 'Gesicht'; tschad. kot. *fné, fene* 'Brust' (Lukas 1937, 147, 154).
- (10) sem. arab. *zahr* 'Rücken'; meh. *ṭahar, ṭar* 'auf'; šaḥ. *zer*; soq. *ṭhar*; berb. *tuar, azir* 'Schulter'; tschad. somr. *tar* 'Rücken' (Lukas 1937, 81; *tarúm* 'dein Rücken').
- (11) sem. akk. *kappu* 'Handfläche'; hebr. *kaf* 'hohle Hand'; meh. *kaff* 'Hand (fläche)'; tschad. sura *cáap* 'Flügel, Feder' (Jungraithmayr 1963, 61); tera *kopax* (-x Suffix, s. Newman-Ma 1966, 242) 'Flügel' (ebd. Nr. 119).
- (12) sem. hebr. *pot* 'Vulva' (Jes. 3,17; zweiradikaliges Nomen vom Typus *qul*, s. Macuch 1969, 379), 'Türangelloch' (1 Kön. 7:50); arab. *faut* 'Zwischenraum';



har. (<kusch.) *fuddi* 'Anus'; sel. *fodo*; kusch. dar. *fido* 'Genitalien' (Leslau 1963, 61); som. *fūto* 'Anus' (s. Cohen 1947, Nr. 381); tschad. jegu *paate* 'Vulva', *paato* 'Penis' (Jungrathmayr 1961, 116); Illič-Svityč (1966), 24, Nr. 2.26 wird ang. *fut* 'Loch' angeführt; vgl. auch ganza *pit* 'Vulva'; gao *buti* (Greenberg 1966, 146).

(13) sem. meh. *šit* 'Vulva, Penis'; šah. *šit* 'Hinterer' (ob zu sem. \**sit* 'Grundlage, Hinterer'? s. Nöldeke 1910, 143f.); kusch. kafa *šittō* 'Vulva'; moča *šitto*; som. *šitto* (Cerulli 1951, 502).

(14) sem. (<kusch.? s. Leslau 1963, 58) har. *dūr* 'Vagina'; kusch. som. *dubur* 'Anus'; tschad. ha. *dūri* 'Vagina'.

(15) sem. soq. *berbéroh* 'Schenkel'; 'omanisch-arab. *barbur* 'Penis' (als Euphemismus); daḡina-arab. *barbūr*; kusch. kafa *borborō* 'Schenkel'.

(16) sem. meh. *šrayn* (= *šr* + *in*) 'Bein, Unterschenkel, Fuß'; soq. *šér'ehan* 'Füße'; tschad. mand. *sérá* 'Fuß' (Lukas 1937, 119); hitk. *sárá* 'Bein' (Lukas 1964, 108); tera *sārə*; tschad. \**s-r-* 'Fuß, Bein' (Newman-Ma 1966, Nr. 36), wozu Hodge (1968, 27) äg. *šn.wj* 'beide Füße' stellt.

(17) sem. \**dam* 'Blut' (Nöldeke 1910, 117ff.), davon vielleicht denominiert \**dm* 'rot sein'; berb. *idammən* 'Blut' (Cohen 1947, Nr. 335); kusch. kafa *damō* 'Blut' (<amh.? s. Cerulli 1951, 429); sid. *dumē*; ga. *dīmā* 'rot'; tschad. maha *dom* 'Blut'; tschad. \**d-N* (*N* = Nasal; Newman-Ma 1966, Nr. 4).

(18) sem. \**bny* 'bauen'; kusch. \**min* 'Haus'; berb. tuar. *ehen* 'Lederzelt' (Rössler 1964, 215); lib. \**bin*, \**bun* 'Haus' (ebd. 200f.); tschad. \**bin* 'Haus', *bn* 'bauen' (s. Greenberg 1966, Nr. 40 sub 'house'; Diakonoff 1965, 47).

(19) sem. har. sel. zwey *gār* 'Haus'; wo. *gar*; kusch. mao *gera* 'Haus'; moča *kàro*; tschad. mubi *gīr* 'Haus' (Lukas 1937, 182).

(20) sem. (<kusch.?) sod. *ṭaba* 'irdenes Gefäß' (Leslau 1968, 69,1); äg. *ḏb.t* 'Ziegel'; kopt. *tōōbē*, *tōbē* (Vollers 1896, 654); arab. *ṭūba*, span. port. franz. engl. *adobe* 'an der Sonne getrockneter Luftziegel'; kusch. som. *ḏōb* 'Schlamm'; moča *duppo* 'Schlamm für die Wandverkleidung'; tschad. \**t-ḡ-* 'Schlamm' (zum Bauen; Newman-Ma 1966, Nr. 69), z.B. ha. *tāḡo*; hona *taḡe* (s. auch Cohen 1947, Nr. 253). Die Zusammenstellung von äg. *ḏb.t* 'Ziegel' mit arab. *ṣabba* 'gießen' (Hodge 1969, 107) scheint zweifelhaft.

(21) sem. (<kusch.?) äth. amh. tē. tñā. *das* 'Schattendach, Hütte aus Zweigen und Laub'; har. *dās*; kusch. som. *dās* 'Schattendach' (Leslau 1963, 59); tschad. bud. *dehī* 'Schattendach, Gemach' (Lukas 1939, 96), *dešī* 'Schatten (als Schutz gegen die Sonne)' (ebd. 97).

(22) sem. arab. *lif* 'Palmfasern, Palmbast'; tschad. log. *lefī* 'Palmblatt' (Lukas 1936b, 105); bud. *lēbī* 'Dumpalmblätter' (Lukas 1939, 117).

(23) sem. hebr. *sə'ōn* 'Schuh'; aram. *sēnā*; äth. *ša'n*; tschad. ban. *àsénú* 'Schuh' (Lukas 1937, 134).

(24) sem. meh. *škī* 'Schwert'; soq. *ško*; kusch. kafa *šikkō* 'Dolch, Messer'; moča *šikko*; šin. *šikkā*; ḡan. *siko*; tschad. ank. *sik* 'Messer'; nga. *soki*; tschad. \**s-G-* (Newman-Ma 1966, Nr. 56).

- (25) sem. äth. *nəway* 'Gefäß' (nach Bergsträsser 1928, 186 zu sem. 'ny); tschad. jegu *náayo* 'Wasserkrug' (Jungraithmayr 1961, 115).
- (26) sem. mittelhebr. *qullit*, *qālāl* 'Krug'; aram. *qalālā*; sod. *kolālit* (Leslau 1968, 69,2); arab. *qulla* 'Wasserkrug'; kusch. moča *qullo* 'Milchkrug'; kafa *qullō* (s. Leslau 1959, 47 für weitere Angaben; für tschadische Entsprechungen s. Greenberg 1966, Nr. 57; Sölken 1967, Nr. 442).
- (27) sem. \**dlw* 'den Eimer heraufziehen, schöpfen' (s. Bergsträsser 1928, 186); akk. *dalū* 'Schöpfemeier'; arab. *dalw*; tschad. dor. *dāli* 'Wassertopf' (Lukas 1937, 91); log. *dālī* (Lukas 1936b, 90).
- (28) sem. akk. *dīqoru* 'Topf, Kochkessel'; ug. *dkrt* (Dietrich-Loretz 1967, 538); aram. *qidrā*; arab. *qidr*; kusch. kafa *qondō* 'Tongefäß' (Cerulli 1951, 486), *kondō* 'Kochtopf aus Ton' (ebd. 462); tschad. mus. *gēdeīāṅ* 'Kochtopf' (Lukas 1941, 56); bar. *kirtu* 'Eßtopf' (Lukas 1937, 50); nang. *dēgele* (ebd. 89); bana *gēlda* 'Wassertopf' (ebd. 130); Newman-Ma (1966), Nr. 137: 'pot<sub>2</sub>' tschad. \**D-gh-l-*, allerdings auf Grund von sechs Formen aus anderen Sprachen erschlossen.
- (29) sem. akk. *dūdu* 'Kochtopf'; hebr. *dūd*; aram. *dūdā*; tschad. ban. *doīda* 'Kochtopf' (Lukas 1937b, 134).
- (30) sem. (<kusch.) äth. *bareta* 'Schüssel' (Varenbergh 1915-1916: 5, 7); amh. *tña. bareta* 'Nachtgeschirr'; tē. *baräyta*; eža *barita* (Leslau 1969, 72, Nr. 100); kusch. kafa *báretō* 'kleines Tongefäß' (Cerulli 1951, 415); tschad. bol. *buri* 'Topf'; Newman-Ma 1966, Nr. 78: 'pot<sub>1</sub>' tschad. \**ḡ-(r-)*.
- (31) sem. \**yam* 'Tag' (s. Nöldeke 1910, 133; Bergsträsser 1928, 185); hebr. *yōm*; arab. *yaum*; kusch. s. Cohen (1947), Nr. 497; tschad. jegu *yóm*, pl. 'ole 'Tag' (Jungraithmayr 1961, 118).
- (32) sem. meh. *karmaým* 'Berg'; berb. *akrum* 'Rücken'; kusch. bil. *kirma* 'Rücken'.
- (33) sem. (<kusch.) amh. *dəngay* 'Stein'; kusch. som. *ḡagaḥ* 'Stein'; ga. *ḡagā*; kafa *taqqō*; tschad. \*(N)d-G- 'Stein' (Newman-Ma 1966, Nr. 104).
- (34) sem. meh. *berōr* und *ḡabrīr*, pl. *ḡabrōr* 'Sand'; kusch. som. *būr'o* 'Sanddüne'; tschad. log. *būrā* 'Sand, Staub' (Lukas 1936b, 87); bud. *būrbur* (Lukas 1939, 93); tschad-arab. *abarbar* 'Staub' (Roth-Laly 1969b, 15), *burūr* 'sandige Gegend' (ebd. 45).
- (35) sem. šaḥ. *gend* 'Holz, Gehölz'; meh. *maḡindōt* '(Baum)stamm'; äth. *gʷənd*; amh. *gənd*, entlehnt in kusch. kafa *gindō*; tschad. somr. *ḡūndo* 'Wurzel' (Lukas 1937, 78).
- (36) sem. arab. *darb* 'Engpaß, Pfad, Weg'; kusch. bil. *dārb*, *dāreb* 'Weg'; saho *darib* (Dolgopolskij 1966, 70, Nr. 5,8 jedenfalls nicht als Entlehnungen aus dem Arabischen angesehen); tschad. bud. *tūrbo*, *tórbu* 'Weg' (Lukas 1939, 128); ha. *turba*; kot. *darba*; tschad. \**t-(r)b-* 'Weg, Pfad' (Newman-Ma 1966, Nr. 86).
- (37) äg. *wj̄t* 'Weg'; kusch. kafa *woretō* 'Weg' (Cerulli 1951, 515); tschad. ank. *war* 'Weg'; gid. *ura*; ang. *ār* (Greenberg 1966, Nr. 59).
- (38) sem. meh. *qole* 'Kürbis' (Müller 1965, 387); altamh. *qal'* (Strelcyn 1968, 44); har. *qulu'*; kusch. kam. *qulā* 'Kürbis' (s. Leslau 1963, 123 mit weiteren kuschitischen Entsprechungen); tschad. kot. *gil/gul/gal* als Stammwörter für 'Kürbis' (Sölken 1967,

Nr. 61); log. *kálhā* 'Cucurbita maxima' (Lukas 1936b, 99), *gil* 'Flaschenkürbis' (ebd. 93).

(39) sem. ug. *rbb* 'Regen'; hebr. *rəbībīm*; amh. *rābārrābā* 'leicht regnen'; kusch. ga. *roba* 'Regen'; som. *roob* (von Cohen 1947, Nr. 299 zu sem. *znm*, *zrm* gestellt); tschad. ha. *rūwā* 'Wasser'.

(40) sem. tē. *nəway* 'Besitz, Vieh'; kusch. beḳ. *nā'i*, *nāy* 'Ziege', *nā* 'Schaf' (s. Cohen 1947, Nr. 463); tschad. mand. *nāwè* 'Ziege' (Lukas 1937, 122).

(41) sem. (<kusch.) äth. *dabela* 'Ziegenbock, Widder, Stier'; tē. *dābela*; tña. *dibāla*; kusch. saho afar *dabēlā* 'Ziegenbock, Widder' (s. Brockelmann 1950, 22 und Dolgopolskij 1966, 73, Nr. 5.23 über mögliche Entsprechungen im Semitischen); tschad. kot. *dumalā*, *tamanā* 'Büffel' (Sölken 1967, Nr. 237f.); bud. *dal* 'Ziegenbock' (Lukas 1939, 96); mand. *dālú* 'Stier' (Lukas 1937, 122).

(42) sem. (<kusch.?) amh. *goš* 'Büffel'; gur. \**gäš*; kusch. cham. *goš* 'Büffel'; cha. *gušā*; sid. *gōše*; ḡan. *gēšā* (kaum mit Cerulli 1938b, 75 von arab. *ḡāmūs*, das ja seinerseits ein iranisches Lehnwort ist); tschad. \**g-s* 'Stier' (s. Newman-Ma 1966, Nr. 11).

(43) sem. äth. *wäyṭāll* 'Gazelle'; tschad. kot. *wutēli*, *wutēri* 'Gazelle' (Sölken 1967, Nr. 58); log. *uteli* (Lukas 1936b, 124); mus. *úderi* (Lukas 1941, 79).

(44) sem. (<kusch.) sod. *g<sup>w</sup>āmāčā* 'Antilope' (Leslau 1968, 78,8); kusch. sid. *gurumiččō*, *gurumiččō* 'Madoqua-Antilope' (Cerulli 1938a, 205); ḡan. *gimwa* (Cerulli 1938b, 75); tschad. \**g-m-k* 'Antilope' (Newman-Ma 1966, Nr. 1).

(45) sem. šah. *qəraḥ* 'Esel' (s. Müller 1968, 365); zum Kuschitischen und Tschadischen s. Greenberg (1966), Nr. 3; Diakonoff (1965), 53; weitere Beispiele tschad. tub. *gerihe* 'Esel' (Lukas 1937, 101); baza *guára* (ebd. 113); somr. *korá* (ebd. 79); kot. *kōro* (Sölken 1967, Nr. 233), trotz der Bedenken von Newman-Ma (1966, 248), daß die tschadischen Formen aus kanuri *koro* entlehnt sein könnten.

(46) sem. (<kusch.) amh. *zāhon* 'Elefant'; har. *doxon*; sel. *dāhano* (Leslau 1963, 55); amh. *žan* 'Majestät' (über den Zusammenhang zwischen *žan* und *zāhon* s. Mittwoch 1911, 281-6); kusch. \**zak<sup>w</sup>an* 'Elefant' (Cerulli 1938b, 88); bil. qua. kem. *ḡānā*; tschad. gab. *jénu* 'Elefant' (Lukas 1937, 87); nang. *jená* (ebd. 89); kaba *junó* (ebd. 92); tschad. \**g-w-n* (Newman-Ma 1966, Nr. 27); z.B. noch tera *juwan*; viz. *giwan*; vgl. auch teda *kuhun* 'Elefant'; maba *ngon* (Greenberg 1966, 138).

(47) sem. tē. *kāray* 'Hyäne'; tschad. ha. *kūrā* 'Hyäne'; tub. *kórne* (Lukas 1937, 101).

(48) sem. meh. *duduwoṭ* 'Wölfin'; tschad. log. *dādú*, *dádu* 'Hyäne' (Lukas 1936, 87); vgl. z.B. hebr. *zə'eb* 'Wolf': amh. *žəb* 'Hyäne'.

(49) sem. (<kusch., s. Brockelmann 1950, 27) äth. *dorho* 'Huhn'; amh. *doro*; tē. *derho*; tña. *dārho*; kusch. \**dir<sup>k</sup>w* 'Huhn' (s. Cerulli 1938b, 166 und Dolgopolskij 1966, 71, Nr. 5.10 mit zahlreichen Beispielen); tschad. somr. *dúrrēi* 'Henne' (Lukas 1937, 78).

(50) sem. (<kusch., s. Brockelmann 1950, 26f.) äth. *zāgra*, *zəgra* 'Perlhuhn'; tña. *zagra*; amh. *žəgra*, *ḡəgra*; har. *zikra*; kusch. saho *zagrā* (s. Cerulli 1938b, 89 für weitere Belege); tschad. ha. *zəkarə* 'Hahn'; mus. *mzəkr* 'Huhn' (Lukas 1937,

143); Newman-Ma (1966), Nr. 39 : 'fowl' tschad. \*(*Q*)*k-r-*, d.h. erster Konsonant nicht genau bestimmbar.

(51) sem. (<kusch.) äth. *sägāno* 'Strauß'; amh. *säg<sup>w</sup>än*; tē. tña. *sägān*; kusch. (s. Brockelmann 1950, 25f.) bil. *sagan* 'Strauß'; cha. *sagunā*; qua. *sagānā*; tschad. kot. *sigli*, *skāle* 'Strauß' (Sölken 1967, Nr. 192); log. *skelē* (Lukas 1936b, 119).

(52) sem. akk. *tultu* 'Wurm'; mittelhebr. *tōla'at*; meh. *tawālēt* 'Raupe'; tschad. log. *tēldi* 'Wurm, Insekt' (Lukas 1936b, 122).

(53) sem. aram. *nā'ā* 'Brust'; meh. *nā'it* 'Euter'; šaḥ. *n'ét*; soq. *ni'ih*; äg. *mnq* 'Brust, Euter' (Gleichung sem. ' : äg. *q* nach Rössler); tschad. somr. *nāē* 'Euter' (Lukas 1937, 80).

(54) sem. (<kusch.) har. *gōga* 'Haut, Fell'; gur. *goga*; kusch. ga. sid. *gōgā* 'Haut, Fell'; kam. *gogá*; gan. *gōgō*; kafa *gokkō*; moča *gōqqo*; tschad. mand. *gògwà* 'Haut, Fell' (Lukas 1937, 119).

(55) sem. (<kusch.) amh. tña. *gamma* 'Mähne' (s. Leslau 1963, 72 für weitere Belege); kusch. ga. sid. *gamma* 'Mähne'; som. saho afar *gama*; tschad. \**g-m-* 'Bart' (Newman-Ma 1966, Nr. 2), z.B. ha. *gēmy*. Hodge (1968, 25) stellt dazu äg. *gmḥ.t* 'Haarflechte'.

(56) sem. \**qibat* 'Labmagen (der Wiederkäuer)' (s. Nöldeke 1910, 155); hebr. *qēbāh*; arab. *qiba*, *qibba*; meh. *qabit* 'Magen'; tē. *qābbāt* 'vollgestopfter Ziegenmagen'; Cohen (1947), Nr. 230bis mit fraglichen ägyptischen und tschadischen Entsprechungen; tschad. sura *kápít* 'Magen' (Jungraithmayr 1963, 70).

(57) sem. (<kusch.) amh. *fandayya* 'Dung, Mist (von Pferd, Esel, etc.)'; gur. *fando*; kusch. ga. *faando* 'Dung, Mist'; som. *faanto*; dar. *fāndò*; bed. *findo* (s. Leslau 1959, 45); tschad. ha. *findi* 'Exkrement' (Dolgopolskij 1966, 59, Nr. 2.5 und bereits Meinhof 1912, 236 unter 'Mist'); die Illič-Svityč 1966, 21, Nr. 2.7 auf Grund von mus. *filái* u.a. angenommene tschadische Grundform \**ph-l/t/-* ist wohl davon zu trennen.

(58) sem. meh. *hanōb* (*h* nicht zur Wurzel gehörig) 'groß, alt' (fem. zu *šēh*); arab. *nāb* 'alte Kamelin'; äg. *nb* 'Herr' (Cohen 1947, Nr. 423 zu sem. *rbb*); kusch. saho afar *nab* 'groß, mächtig'.

(59) sem. amh. *bado* 'leer'; tschad. log. *bútu* 'leer, nackt' (Lukas 1936, 87).

(60) kusch. gan. *forō* 'weiß' (Cerulli 1938b, 73); tschad. ha. *fari* 'weiß'.

(61) sem. arab. *ǧalla* 'groß, erhaben sein'; tschad. masa *gal* 'groß' (Lukas 1937, 98); hitk. *gùlù* 'groß', *gul-* 'übertreffen' (Lukas 1964, 107).

(62) sem. \**ml* 'voll' (s. Bergsträsser 1928, 190); tschad. mus. *mulf* 'schwanger' (Lukas 1941, 69).

(63) sem. meh. *dīnī* 'schwanger sein'; šaḥ. *dīni*; soq. *dīnih*; gur. (hierher gehörig?) čaha *dānam*; enn. *dān'a*; tschad. sura *dùn* 'nicht hohl, voll (von der weiblichen Brust)' (Jungraithmayr 1963, 63).

(64) sem. meh. *zay*, *ḍay* 'Geruch, Gestank'; soq. *ṭay*, äth. *ši'at*; berb. *aḍu* 'Geruch'; tschad. sura *du* 'Geruch, Duft' (Jungraithmayr 1963, 65).

(65) sem. meh. *wōl* 'Entzündung (der Augen)'; tschad. bud. *wel*, *wil* 'schmerzen'

(Lukas 1939, 130); kot. *wali* 'Krankheit' (Sölken 1967, Nr. 528), *siɣwal* 'Augenentzündung' (ebd. 538).

(66) sem. hebr. *lāham* '(Brot) essen', *lāhām* 'Brot'; arab. *lahm* 'Fleisch'; tschad. somr. *lāma* 'schmecken, kosten' (Lukas 1937, 80); mus. *lāma* '(feste Speise) essen' (Lukas 1941, 64).

(67) sem. \**t*'*m* 'kosten, schmecken'; kusch. bed. *tam* 'essen', s. Diakonoff (1965), 46; Cohen (1947), Nr. 337; Dolgopolskij (1966), 86, Nr. 8.14; tschad. bud. *tam* 'schmecken' (Lukas 1939, 126).

(68) kusch. kafa *uč* 'trinken'; ġan. bad. bask. čara zay. šin. gim. *uš* (Cerulli 1951, 393-4); tschad. ha. *šā* 'trinken'; mand. *ša*; tschad. \**s*- (Newman-Ma 1966, Nr. 23).

(69) sem. meh. *tīq* 'trinken'; arab. *ta'īqa* 'voll sein', (IV) 'anfüllen'; kusch. ga. *duḡa* 'trinken' (Meinhof 1912, 239 mit anderen Vergleichen); bur. *tiɣ*; tschad. sok. *tāgo* 'essen'.

(70) sem. hebr. aram. *mrā* 'reiben, glätten'; arab. *marāḡa* 'einölen, salben'; kusch. ga. *morā* 'Fett, Talg'; tschad. \**m-r* 'Öl' (Newman-Ma 1966, Nr. 74), z.B. *tera gudu mar*; nga. sura *mor*. Hodge (1968, 26) vergleicht äg. *mrḥ.t* 'Öl'.

(71) sem. arab. *ġarā* 'laufen'; meh. *ġerū* 'vorbeigehen'; tschad. gab. *gére* 'laufen' (Lukas 1937, 88); nang. *gūr* (ebd. 89); dor. *geré* (ebd. 91); log. *gār* 'gehen' (Lukas 1936b, 93).

(72) sem. arab. *fāta* (*fūt*) 'entschwinden, vorübergehen'; kusch. s. Dolgopolskij (1966), 67, Nr. 3.21; tschad. ang. *pūt* 'hinausgehen, ausgehen'; sura *pūt* (Jungraithmayr 1963, 79); chip *pūt* (Jungraithmayr 1964, 178); bol. *pete*; musg. *put*; ha. *fīta*; tschad. \**p-t* 'hinausgehen' (Newman-Ma 1966, Nr. 44).

(73) äg. *lw*, *ij* 'kommen'; berb. *ayu* 'kommen'; kusch. beḏ. *yi*, *i* 'kommen' (Cohen 1947, Nr. 25); ergänze kafa čara *wā*; moča *wāa*-; šin. *wa*; gim. *wo*; tschad. log. *àwà* 'kommen'; bud. *u(wùu)*. Mukarovskij (1969, 344) rekonstruiert für das Kuschitische und Tschadische eine Grundform \**wāa*, *àwú*.

(74) sem. 'ly 'aufsteigen' (s. Bergsträsser 1928, 187); äg. berb. kusch. s. Cohen (1947), Nr. 58; Diakonoff (1965), 48; tschad. mon. *laa* 'aufgehen (Sonne), aufstehen' (Jungraithmayr 1964, 178); log. *lá* 'aufheben' (Lukas 1936b, 104).

(75) sem. tñā. *g<sup>w</sup>āḡalā* 'stehlen' (akk. *kalū* nicht 'nehmen', so Cohen 1947, Nr. 190, sondern 'zurückhalten', Wurzel *kl*); kusch. beḏ. *g<sup>w</sup>āhar* 'betrügen, stehlen' (Rössler 1964, 210); tschad. hitk. *kāl-* 'nehmen, heiraten' (Lukas 1964, 107); für weitere Verben mit der Bedeutung 'stehlen' s. Greenberg (1966), Nr. 71. Vgl. auch barea *hol* 'stehlen'; shilluk *kwalo*; dinka *kwāl*; bari *kola* (Greenberg 1966, 105).

(76) kusch. sid. *mor*, *moḏ* 'stehlen' (Cerulli 1938a, 213); tschad. \**m-r* 'stehlen' (Newman-Ma 1966, Nr. 101).

(77) sem. nordsem. *prq* 'zertrennen'; arab. *faraqa* 'teilen', *tafaraqa* 'sich zerstreuen'; tschad. log. *peraka-ze* 'sich zerstreuen' (Lukas 1936b, 115).

(78) sem. syr. *pak* 'zerbrechen'; arab. *fakka* 'öffnen, lösen, freilassen'; kusch. bil. *fakak* 'öffnen'; beḏ. *fekik*; saho afar *fak*; von Cohen (1947), Nr. 362 zu sem. *fḡy*, *pqḡ* gestellt; sid. *bek* 'teilen, trennen'; tschad. sura *ḡàk* 'teilen, trennen, auseinander

gehen, (sich) loslösen' (Jungraithmayr 1963, 59). Vgl. auch mahas fadidja *fag* 'spalten'; dongola *bag* (Greenberg 1966, 105); kunama *fak* (ebd. 126).

(79) kusch. kafa *kār* 'erzürnen', kaus. 'streiten', *kārō* 'Zorn'; moča *kàri-ye* 'streiten', *ka'ro* 'Haß, Streit'; qua. *kāri* 'streiten'; tschad. \**k-r-* 'streiten' (Newman-Ma 1966, Nr. 83), z.B. tera *kara*.

(80) sem. akk. *dāku* 'töten, schlagen'; hebr. aram. *dūk* 'zerstoßen'; arab. *dāka* und *da'aka* 'zerreißen'; tschad. \**D-k-* 'töten' (Newman-Ma 1966, Nr. 53), z.B. kare. *duk*; vgl. auch ha. *dōkq* 'schlagen' (bei Newman-Ma 1966, Nr. 122), das Greenberg (1966), Nr. 5 mit kuschitischen Entsprechungen zu sem. *dqq* 'zerstoßen' stellt. Vgl. auch Cohen (1947), Nr. 340.

(81) sem. akk. *qālu* 'rufen'; arab. *qāla* 'sagen'; hebr. *qōl* 'Stimme, Ruf'; kusch. sid. *kul* 'sagen' (Cerulli 1938a, 209), *qol* 'antworten' (ebd. 216); tschad. jegu *kol-* 'nennen, rufen' (Jungraithmayr 1961, 114); gab. *guāl* 'sprechen' (Lukas 1937, 88). Vgl. auch daza *kul* 'rufen' (Greenberg 1966, 136).

(82) sem. arab. *rawā* 'berichten, erzählen'; meh. *ruwé* 'Gesang'; šaḥ. *re* 'singen, Gesang, Lied'; soq. *re* 'verkünden'; tschad. jegu *rèyé* 'Gesang, Lied' (Jungraithmayr 1961, 116).

(83) sem. äth. *na* 'siehe!'; äg. und berb. s. Cohen (1947), Nr. 465; tschad. sura *nāa* 'sehen' (Jungraithmayr 1963, 76); chip *nāa* (Jungraithmayr 1964, 180); tschad. \**n-* 'sehen' (Newman-Ma 1966, Nr. 90); Greenberg (1966), Nr. 62 vergleicht sem. arab. *inna* 'wahrlich', hebr. *hinn-* 'siehe!'.  
(84) kusch. sid. *la* 'sehen'; had. kam. *la* 'wissen'; tschad. \**l-* 'sehen' (Newman-Ma 1966, 91).

(85) sem. meh. *šīnī* 'sehen'; šaḥ. *šené*; soq. *šīnī*; äg. *šwn* 'wissen'; berb. \**šwn* 'wissen' (Rössler 1964, 207); tschad. ha. *sanī* 'wissen'; log. *sán*; bud. *hin*, *hine* (s. Greenberg 1966, Nr. 42); sura *sēen* 'Klugheit, Weisheit' (Jungraithmayr 1963, 81); Newman-Ma 1966, Nr. 57: 'wissen' tschad. \**Z-n-*, z.B. noch mar. *səni*; gid. *sən* und weitere Entsprechungen. Diakonoff (1965, 48) stellt mit Fragezeichen sem. arab. *waṭan* 'Idol' hierzu; vgl. zu den von mir vorgeschlagenen Etymologien indogerm. \**ueid-* 'sehen, wissen', z.B. deutsch *wissen* und lat. *videre*.

(86) sem. aram. *trēn* 'zwei'; meh. *tarō*, *tirú*; šaḥ. *teró*; soq. *tiró*; sonst sem. \**tin-* (s. Bergsträsser 1928, 191); äg. und berb. s. Diakonoff (1965), 49; tschad. mubi *šīr* 'zwei' (Lukas 1937, 185); somr. *sir* (ebd. 81); jara *səre*; musg. *sray*; tschad. \**s-r-* 'zwei' (Newman-Ma 1966, Nr. 113), bereits von Hodge (1968, 27) zu sem. \**tn* gestellt.

(87) sem. äth. *gesā* 'morgens aufstehen', *gesām*, *gešām* 'morgen'; tschad. kot. *gīsu* 'morgen' (Sölken 1967, Nr. 62).

(88) sem. meh. *bō* 'hier'; šaḥ. soq. *bo*; hebr. *poh*; amh. *bota* 'Ort'; ägyptische, kuschitische und tschadische Entsprechungen in der Bedeutung 'Ort' s. Greenberg (1966), Nr. 56; Illič-Svityč (1966), 17 bzw. 19, Nr. 1.33.

(89) sem. meh. *ḥalōk* 'dort'; tschad. somr. *áloka* 'dort' (Lukas 1937, 77).

(90) sem. arab. *warā'a* 'hinter'; meh. *wurā* 'zurückkehren'; tschad. \**w-r-* 'Nacken' (Newman-Ma 1966, Nr. 71), z.B. ngiz. gudu *wura*; jegu *were*.

(91) sem. meh. *ka* 'mit'; šaḥ. soq. *ke*-; tschad. log. *ká, ká* 'mit' (Lukas 1936b, 99); jegu *ka* (Jungraithmayr 1961, 113). Vgl. aber auch shilluk, lango und lotuko *ka* 'mit' (Greenberg 1966, 108); daza *kē*; fur *ki*; gumuz *ka* (ebd. 147).

(92) sem. yemenitisch-arab. *dow, dā, dawwa* Negationspartikel (s. Wagner 1966, 263); tña. *-do* Fragepartikel; kusch. had. sid. al. maḡ. gim. kafa *-t (-ti, -te)* negativum (s. Plazikowsky-Brauner 1957, 23f.); tschad. kot. *dō* Negationspartikel (Lukas 1937, 150); mubi *-dò* (ebd. 181); jegu *(-)dó, (-)to* (Jungraithmayr 1961, 111); log. *dá* Frageadverb (Lukas 1936b, 87).

Abgekürzt zitierte Namen von Sprachen (nach Sprachgruppen getrennt):

sem(itisch), akk(adisch), amh(arisch), arab(isch), aram(äisch), (alt)südarab(isch), äth(iopisch) = ge'ez, enn(emor). gur(age), har(ari), hebr(äisch), meh(ri), sel(ti), šaḥ(ri), sod(do), soq(oṭri), syr(isch), t(igr)ē, t(igrīñ)ña, ug(aritisch), wo(lane);

(alt)äg(yptisch), kopt(isch);

berb(erisch), lib(ysch), tuar(eg);

kusch(itisch), al(aba), bad(itu), bask(eto), beḡ(auye) = beḡa, bil(in), bur(ungi), cha(mir), cham(ta), dar(asa), ga(lla), ḡan(ḡero), gim(ira), had(iya), kam(batta), kem(ant), maḡ(ḡi), qua(ra), sid(amo), šin(aša), som(ali), wol(amo), zay(sse);

tschad(ohamitisch), ang(as), ank(we), ban(ana), bar(ein), bol(ewa), bud(uma), dor(mo), gab(ere), gid(der), ha(usa), hitk(alanci), kar(bo), kare(kare), kot(oko), log(one), mand(ara) = wandala, mar(ḡi), mod(gel), mon(tol), mus(gu), musg(oi), nang(ire), nga(mo), ngiz(im), sok(oro), somr(ai), suk(ur), tub(uri), viz(ik).

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## DISCUSSION

MÜLLER : Two factors have contributed to the writing of this paper, for the somewhat heterogeneous and unsystematic character of which I should perhaps apologize. Firstly, in a course given at Tübingen University during the last term on problems of Hamito-Semitic linguistics, I examined the word lists given by Diakonoff and by Greenberg and attempted to enlarge them in one or other direction. Thus I believe to have found for instance in Šahri *qérah* a Semitic equivalent of the words for 'donkey', which are very widespread and well represented in Chadic and Cushitic. On the other hand, since I am working on an etymological dictionary of Mehri I have endeavoured to make use for this purpose of the lexical material of New South Arabian, and I think I have succeeded in finding equivalents in the wider African sphere for a whole series of Mehri words which do not appear to possess cognates within Semitic in a narrower sense. Although I believe I have found a series of good new comparisons, I also wish to stress that all the comparisons proposed in this paper are at present merely suggestions and that I would be very grateful for criticisms from your side. I am directing this request especially towards those Africanists who are familiar with Chado-Hamitic.

LESLAU : Without entering into the details of this very complicated problem of comparisons and etymologies, a more general question to my friend Müller. Semitics are relatively well off when it comes to phonetic correspondences, although there are of course problems, but are we as well off with regard to Chadic? I am not a specialist and do not know the answer to this. But to give just one example—'Ziege'

(No. 37 of the circulated version, since dropped). The root is in Semitic an emphatic *t* and *l* and *y*, let's say. Now Müller compares Chadic *tu*. But as long as we don't know really what the phonetic system of Chadic is I could very well compare it with say *se*, that is a sibilant or perhaps an interdental becoming a dental in Chadic. This may be the case, but we would like to be in a position to judge the more general picture of Chadic phonology. And just one more example to illustrate—No. 54 (of the circulated version, cf. now No. 59): Semitic Mehri *faṭá* 'nackt', similarly Šaḥri, Soqotri and so on, Chadic *bútu*. Again, as long as we do not know what the correspondences are between the labials and the dentals I could very well imagine that Chadic *bútu* could be compared with Amharic *bado* 'empty', and so on. So what I mean, to say it again, is: I wish we had—perhaps we have it, but I don't know—more knowledge about the phonetic correspondences within Chadic and only then can we make these comparisons between the various groups.

JUNGRAITHMAYR : I think there is nothing to be added to Professor Leslau's remarks from a general point of view, especially since we have to admit that—apart from Paul Newman's attempt at reconstruction on the basis of about 35 out of approximately 115 Chadic languages, which is entirely provisional—we have not any basis on which we could reconstruct the Chadic proto-forms phonologically. From this point of view there is no doubt that comparison between any individual languages of the Chadic group on the one hand and any individual language of any other sub-family of Hamito-Semitic on the other would be unwarranted. I can however see at least one aspect of value in this paper and that is—very general at first—that this direction of comparison particularly of Southern Semitic, or Cushito-Semitic, areas on the one hand and Chado-Hamitic, or Chadic, on the other hand has so far been rather neglected, so that the examples or proposals given here have at least a certain value of stimulation. But beyond that, I do not think that we can yet at this stage build on what my colleague and friend Dr. Müller has proposed. However, in concluding, I would like to draw your attention to one probable cognate, which I do not remember whether it is included or not, that is the gloss for 'to eat'. Newman has reconstructed *\*t-*, as we have Hausa *čī* and so on in many other Chadic languages. On the other hand we have a form *tuwó* in Mehri. Now since in certain Chadic languages two basic stems of this verb occur, as has been pointed out especially for Mubi by Lukas in the thirties and recently by myself for the Ron languages on the Plateau of Northern Nigeria, the situation presents itself in the following way. We have on the one hand the short aoristic stem *ti*, which would form the basis for reconstructing proto-Chadic *\*t-*, on the other the long stem *tua* or *tuai*—cf. Hausa *tuo* as a nominal form—and we have in Mubi *te* as the short stem in contrast to *tua* as the long stem. So this fact, namely the existence of certain 'mutable stems' as we might call them following Dr. Andrzejewski, in some remaining languages might point to the possibility that we have to go beyond Chadic to reconstruct for instance the word for 'to eat' not as *\*t-* but rather as *\*twy-*.

## A HISTORICAL DESCRIPTION OF THE HEBREW ROOT YŠ'

JOHN F. A. SAWYER

0.1. The term 'root' is an abbreviation for 'root-morpheme', and denotes an identifiable group of consonants, not necessarily contiguous, which are common to a number of lexical items. 'YŠ'-terms' and 'YT'-terms' are shorthand expressions intended to avoid such cumbersome and possibly misleading phrases as 'items containing the element YŠ'' or 'words derived from the root YT''.

0.2. As well as the usual general problems involved in Semitic historical linguistics—the role of chance in the survival of many of our sources, the inadequacy of some of the scripts in which they were written, the backlog of several centuries of erroneous linguistics—several particular problems are raised by the present subject.

0.2.1. There is the phonological problem that each of the three consonants of the root YŠ', namely, /j/, /ʃ/, and /ç/, may correspond to at least two Proto-Semitic consonants, which in turn may have developed independently in the other Semitic languages. This brings the possible number of cognate forms throughout the language group to over twenty, not counting forms due to conditioned phonetic changes and inadequate graphic representation.

0.2.2. The semantic problem is that the contexts in which the YŠ'-terms occur are peculiarly limited in range. A survey of their immediate linguistic environment in Biblical Hebrew shows that they normally collocate only with the name of a god or a religious leader. Consideration of their wider context confirms this: at all periods right up to the present day, these terms are extremely rare outside the context of definitely religious language.

0.2.3. There is also, what might be called, a folk-linguistic problem. The eighteenth century French philologist Schultens (1761) noticed a similarity between Hebrew YŠ' and Arabic WS'. At the phonological level the relationship was possible; the morphological correspondence was convincing (Hebrew causative *hošia'* beside Arabic causative *'awsa'a*). But it was undoubtedly the semantic correspondence which was the decisive factor: Arabic *'awsa'a* 'to give room to' provided the key to the 'literal' or 'original' meaning of Hebrew *hošia'* 'to save' (cf. Sawyer 1968, 20, notes 1, 2). The etymology has been enthusiastically accepted by the majority of

Biblical scholars ever since. The evidence now available, however, suggests a different etymology. As long ago as 1907 the standard Hebrew and English Lexicon of the Old Testament recorded the difficulty in accepting Schultens' theory (Brown, Driver and Briggs 1908, 446), and, although more recent lexica have ignored the evidence and given lexicographical authority to the *wasi'a* theory (Kittel 1960-1964, 973 f.; Koehler and Baumgartner 1953, 412), a quite different linguistic history is presupposed for Hebrew YŠ'-terms in works on Amorite and Ugaritic.

## 1. THE SEMITIC ROOT YT'

The suggestion that Hebrew YŠ' corresponds to YT' in several other Semitic languages is based on the following facts.

1.1. In two other North West Semitic languages and all the Epigraphic South Arabian languages YT'-terms occur in a number of proper names which closely correspond to Hebrew names containing YŠ'-terms.

1.2. Wherever YT'-terms are attested, they collocate with names of deities, a feature of Hebrew YŠ'-terms already referred to (0.2.2.).

1.3. The chronological evidence is consistent and convincing as the historical description below proves.

1.4. Those who defend the only alternative theory so far proposed can point to no evidence at all apart from obvious phonological and rather less obvious semantic resemblances. The argument for the *wasi'a* etymology, in other words, is no more convincing than that for a historical connexion between Hebrew *hošia'* and, let us say, Arabic *waṭaḡa* 'to break, wound', hence Hebrew *hošia' le* 'to fight on behalf of' (cf. 2.3.).

1.5. The history of Arabic *wasi'a* would probably begin with Egyptian *wšḥ* (causative *ššḥ* 'to widen, enlarge'), and clearly develops independently. The correspondences Egyptian *š*: Semitic *t*, and North West Semitic *t*: Arabic *s* would be unusual.

## 2. THE EVIDENCE OF PROPER NAMES

2.1. Outside Hebrew it is remarkable that all the YT'/YŠ'-terms occur only in proper names. Before we can use this special type of evidence several preliminary points must be made.

2.1.1. The occurrence of a personal name in a particular set of documents does not

prove that any or all of its separate elements were productive in the region where the documents were found. The appearance of the name *yī'd* (*yāṭa'* + *addu*: 3.1.1.), for example, in the Ugaritic texts does not entitle us to speak of a Ugaritic verb *yāṭa'*.

2.1.2. The occurrence of a personal name in documents dated to a particular period does not prove that any or all of its separate elements were productive in the language of that period. The unusual persistence of proper names, unchanged for centuries, suggests that even the most common names may be made up of elements which have long been unproductive in the language of the people who bear them.

2.1.3. Sound changes and grammatical peculiarities are attested in the proper names of a language which are not found at all or very rarely elsewhere in the same language. This is tacitly assumed in discussions of certain sound changes (e.g. final /ç/ > ø; Gröndahl 1967, 147), shortened forms (Noth 1928, 155 et pass.), and the survival of case-endings (Huffmon 1965, 104 ff.). Whether this is due to a chronological discrepancy between proper names and the rest of the linguistic data (cf. 2.1.2.), or to the peculiar structure of names, or to the effect of folk-etymologies which are particularly common in onomastics, or to some other cause, it is evident that allowances must be made for anomalous developments in proper names.

2.1.4. Frequency depends on factors not always amenable to us: for example, a name recurring very frequently in a number of short texts may either indicate that this was a common name or that one particular individual (possibly with a relatively rare name) is mentioned frequently in the same texts.

2.1.5. Finally, while the forms reconstructed in Amorite proper names still closely resemble some of the Hebrew forms (3.1.1.; cf. 3.2.2.), the assumption that their meaning has also remained unchanged (e.g. Huffmon 1965, 215) is not justified. There is a suggestion that in the semantic history of the Hebrew YŠ'-terms there has been a development from a forensic sense ('to defend from injustice') to a more general sense ('to save, help') (3.2.5.). This would imply that an Amorite verb *yāṭa'* did not have the general sense of 'help', but a more particular forensic sense. However that may be, the general point that phonological or grammatical conservatism in a language is not necessarily an indication of semantic conservatism is worth making, especially in a discussion of terms often explained by reference to Arabic.

2.2. There are some statements, however, on the evidence of proper names which can usefully be made in the present discussion.

2.2.1. In the first half of the second millennium B.C. Amorite stands out as the only Semitic language in which YT'-terms are attested. If there were only one or two proper names containing these terms, as is the case at Ugarit, then there would hardly be any point in attempting to trace the history of the Hebrew YŠ'-terms back before the earliest layers of Biblical tradition. But the names containing YT'-terms are relatively numerous in Amorite and in no other contemporary language.

2.2.2. There is what looks like a parallel situation in Hebrew. On the one hand

no less than 27 individuals with names containing  $Y\check{S}$ ‘-terms are mentioned in the Bible; on the other hand the four main  $Y\check{S}$ ‘-terms occur very frequently in Biblical Hebrew outside the proper names. It is dangerous to press such a parallel too far, especially in the total absence of any evidence; but it does seem at least possible that the frequency of the  $YT$ ‘-terms in Amorite personal names is an indication of the productivity of these terms in their language in general—at some time in its history.

2.2.3. The situation in Epigraphic South Arabian is again similar: a high frequency of names containing  $YT$ ‘-terms, but no evidence as to whether these terms were productive in the language in general.

### 3. HISTORICAL DESCRIPTION

The history of the root  $YT$ ‘ may be divided roughly into three periods: (1) early period from c. 2000 to 1000 B.C.; (2) middle period from c. 1000 to 300 B.C.; (3) late period from c. 300 B.C. to the present day. The evidence for each period will be presented, the chief phonological and grammatical developments noted, and some tentative semantic conclusions drawn.

3.1.1. Early Period. The earliest attested occurrence of the root  $YT$ ‘ is probably in an Amorite personal name written *la-šu-il* in a short legal document from the Third Dynasty of Ur, dated c. 2048 B.C. (Buccellati 1966, 165). Analysable as *la-yašu‘-il*, the name contains a verbal element corresponding to *yašuḥ-/ešuḥ* in eight Amorite names from Mari (c. 1750-1694 B.C.) (Huffmon 1965, 215 f.). The verb appears again in the personal name *yī‘d* (in a cuneiform transcription *ya-aš-ad-du*) in a Ugaritic text of the fourteenth century B.C. (Gröndahl 1967, 147). There is also the name *yīl* in another Ugaritic text from the same period which may be a shortened form of *yī‘l* (2.1.3.). *ya-šu-ia*, the name of the leader of a revolt in southern Palestine, referred to in an Amarna letter, may be another fourteenth century B.C. example of a name containing the same verb (Knudtzon 1915, II, 1319).

3.1.2. The Ugaritic evidence clearly shows that the second consonant of the root was  $t / \theta /$ , written *š* in Akkadian cuneiform (Huffmon 1965, 302; Gröndahl 1967, 14, par. 13); and that the final consonant ‘  $/q/$  was written *ḥ* in Mari cuneiform (Gelb 1958, 2.7.3), but not represented in post-vocalic position at Ur (Buccellati 1966, 165: cf. *yada-DINGIR*). We then have sporadic but consistent evidence for a verb *yaṭa‘* (imperfect *yaṭu‘*: Gelb 1958, 3.3.8.1.1) in North West Semitic from c. 2000 B.C. down to the fourteenth century B.C. It occurs only in personal names where it normally collocates with the name of a deity.

3.1.3. The problematical element *išḥi-* in ten other personal names attested at Mari and elsewhere during the first half of the second millennium B.C. may be related (Albright, Birot, Aistleitner), although some maintain that it is non-Amorite (Bauer, Noth, Gelb; see Huffmon 1965, 215). It could be a shortened verbal form with the connecting vowel *-i* (cf. Noth 1928, 33-36; Huffmon 1965, 106).

3.1.4. As for the meaning of the verb *yaṭa'*, there is no justification for assuming that it necessarily always denoted 'to save, help' (Huffmon 1965; Buccellati 1966; Gröndahl 1967). There is a gap of up to 1000 years between the earliest occurrence of the term and the Biblical evidence where the sense 'to help, save' is first attested (2.1.5.).

3.2. Middle Period. By the end of the second millennium B.C. the sound change  $t > š$  ( $/θ/ > /ʃ/$ ) in the Canaanite languages divides the data into two areas: the *š*-area covers Hebrew, Moabite and Hebrew borrowings in Aramaic, whilst the *t*-area covers the Epigraphic South Arabian languages and Aramaic. The two areas have several features in common.

3.2.1. The high frequency of proper names containing the YT'/YŠ'-terms. At least twenty different names, some of them very common, are recorded from the South Arabian inscriptions (Ryckmans 1934-1935, II, 10, 75 f.). The earliest example is probably Iti'amra (*yīti' 'amara*), the name of an eighth century B.C. Sabaeen king mentioned in the Annals of Sargon (van Beek 1961, 228). There is also one name recorded in a Nabataean text (Cantineau 1930-1932, II, 105). In Hebrew 14 names are found in the Bible and contemporary documents, and of these, two occur in an Aramaized form in the Elephantine papyri (Noth 1928, 154 f., 176).

3.2.2. The appearance for the first time of a causative stem. In Hebrew, alongside the simple stem *yaša'* and the biform *šua'* (Noth 1928, 154 f.) which occur only in personal names (cf. Noth 1928, 36), the causative stem *hošia'* (passive *noša'*) is one of the commonest terms for 'to help, save' in Biblical Hebrew, as well as the verbal element in two common personal names. The verb occurs also in the Moabite inscription of the ninth century B.C. (Segert 1961). In Sabaeen two personal names are recorded with the element *hayta'*- (Ryckmans 1934-1935, I, 232) or *-hayti'* (id., 112), the corresponding causative form.

3.2.3. The predominantly religious application of the YT'/YŠ'-terms. In Biblical Hebrew the usage of these terms is mainly restricted to passages in the historical narratives describing divine or divinely inspired intervention, the language of prayer and religious poetry. Well over a third of the total number of Biblical occurrences are in the Psalms. The god of Israel is, almost without exception, the only subject of actions described by these terms. In this respect there is a clear opposition between *hošia'* and other terms with a similar range of meaning. In South Arabian no other context is recorded for the YT'-terms. On the one hand there are the verbs which collocate regularly with divine names (cf. Ryckmans 1934-1935, I, 232); while on the other hand, we have the divine names *Yiṭa'um* (cf. Ryckmans 1934-1935, I, 6; Höfner 1961, 479 f.), and *Yaṭi'* (Ryckmans 1934-1935, I, 112).

3.2.4. There are no indications of the meaning of these terms in South Arabian, although the parallel forms in Hebrew make it possible that they denote 'saviour, help' (Ryckmans 1934-1935, I, 6).



3.2.5. We come now to the evidence for a semantic development in the Hebrew YŠ'-terms from a technical to a non-technical sense.

3.2.5.1. First there is the peculiar phenomenon, already referred to more than once, that the YŠ'-terms are rarely applied to any act of saving or rescuing other than god's saving Israel from injustice or oppression. An original forensic sense of defending in a court of law, championing justice, would help to explain this.

3.2.5.2. Secondly, the terms are used in explicitly forensic situational contexts: Deut. 22:27; II Sam. 14:4; II Kings 6:26; Pss. 17:7; 60:7 = 108:7; 109:1; Job 5:4. They are associated with terms for reparation (Prov. 20:22), blood-guilt (I Sam. 25:26, 31, 33; Prov. 28:17), ransom (Isa. 43:3), and legal defence (Jud. 6:31; Isa. 19:20; 43:11; 45:20 ff.; Job 5:15 f.; 13:16 ff.).

3.2.5.3. A forensic origin explains the usage of the noun *mošia'* both in Biblical and post-Biblical Hebrew. This may at one time have been the technical term for 'counsel for the defence' (Sawyer 1965).

3.2.5.4. There are obvious parallels to this development: *ga'al* 'to redeem', *pada* 'to ransom' and *šapaṭ* 'to judge' are all applied in contexts of general rescuing or helping, and *šedeq* 'justice' is translatable as 'victory, success' in some contexts.

3.2.5.5. The frequency of legal terms in Semitic proper names provides another useful parallel: e.g. Hebrew Padaiah, Yigael; Ugaritic *dnil*, *tpṭb'l*; Assyrian Ashurdan, Nergalshaphat; Sabaeen Yadān.

A forensic origin for Hebrew YŠ'-terms is certainly possible, but until a wider range of contexts, particularly for the early period, is forthcoming, the question must remain open.

3.3. Late Period. In the later sections of the Hebrew Bible the YŠ'-terms become noticeably less frequent. Apart from the Psalms, direct quotations and a few deliberate archaisms (e.g. Neh. 9:27), they do not occur at all after the fourth century B.C. (Cant., Ruth, Eccl., Esther, Dan., Ezr., Neh., Chron.). It appears that as Hebrew was superseded by Aramaic, these terms became no longer productive outside stereotyped liturgical language and theologoumena. It is significant that, apart from Samaritan (Petermann 1873, 50), the YŠ'-terms are rarely found as loan-words in Aramaic. In the sectarian Hebrew literature of the Qumran community (Kuhn 1960, 94-95), the rabbinic literature (Levy 1924; Jastrow 1903) and Modern Hebrew (Scharfstein 1964) they survive only in liturgical formulae and various expressions consciously derived from the Bible.

3.3.1. One term which carried the root YŠ' into many languages in many parts of the world is the liturgical formula *hoša'na*. From its earliest occurrence up to the present day, where it has been borrowed by most of the languages of the world, the original verbal form functions like a noun, and its original semantic content ('save!') has been overlaid by its contextual function in the liturgical situations for which it was prescribed. Early Christian writers, in discussing the term, show that



they were unaware of its original supplicatory force and believed it to be a shout of jubilation (Thayer 1899).

3.3.2. Finally an interesting semantic shift in the YŠ'-terms is attested from the eighteenth century, when the resemblance between Hebrew *hošia'* and Arabic *'awsa'a* was apparently first noticed (0.2.3.). The meaning of all the YŠ'-terms then came to be defined, in many modern Biblical studies, commentaries and dictionaries, by reference to an 'original meaning' or 'true sense': the verb *hošia'*, for instance, is understood as "to work salvation, *yeša'*, in the true sense of the word, 'width, spaciousness' " (Mowinckel 1959, 47).

#### 4. GENERAL OBSERVATIONS

4.1. The relative instability of the three consonants in the root YT' (0.2.1.) did not prevent us from tracing its development, with a fair degree of certainty, over a period of nearly 4000 years. This is a measure of the priority of contextual, chronological, and geographical data in comparative linguistics over phonological or grammatical equations.

4.2. The line of semantic development which we have attempted to follow begins with technical precision (defence in a court of law: 3.2.5.), and runs into a set of general, almost doxological expressions for divine intervention (help, salvation). The terms are then fossilized in liturgical and devotional language (3.3.), until the European Enlightenment when ingenious comparative philology filled them with a new meaning which they carry up to the present day (spaciousness, freedom: 3.3.2.). At every stage in this history any or none of the previous stages may have influenced the meaning of these terms, depending on the linguistic interests and abilities of the writer or reader. To early Christian writers the previous history of the term *Hosanna* was of no interest (3.3.1.), while to the authors of the Babylonian Talmud the historical meaning of the separate elements in the name *Joshua* was (Soṭa 34b; Levy 1924; Jastrow 1903). Schultens' (1761) 'etymologizing' (0.2.3.), like that of his modern disciples, might even be called synchronic, since it depended more on comparisons with the contemporary linguistic data at his disposal than on the history of the terms he was examining.

4.3. The area in which YT'-terms have been identified is remarkably circumscribed (Syria, Palestine, Arabia). This may be simply due to the problems involved in identifying YT'-terms in other Hamito-Semitic languages (0.2.1.). In Egyptian, for example, the root YT' might appear in various forms, and careful historical investigations would have to be carried out (which the present writer is unable to undertake) before any conclusions could be drawn. The same is true of a suggestion made some years ago (Sawyer 1965, 485) that Ethiopic *'awsē'a* 'to answer' (*tawas'a*

'to defend one's cause') was related to Hebrew *hošia'*. The phonological correspondences are just possible and the association of *hošia'* with *'ana* 'to answer, testify', together with its possible forensic usage in Biblical Hebrew (3.2.5.), makes a connexion with *'awsē'a* still more attractive. But in the last analysis, a decision on this, and on any other theory, depends on contextual and chronological evidence: is the term commonly used in the context of religious language? does it occur in proper names? is it possible that the term was borrowed while one of the YŠ'-terms was still productive?

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## DISCUSSION

FENTON: I might say, in the absence of anything else being said, that I find this paper totally convincing. I think 'awsa'a as the etymon of what we must now regard as YT' has been buried, and very effectively, and I think that the breadth of evidence brought to bear on this from semantic fields and purely linguistic fields is most convincing.

SAGGS: Like Dr. Fenton, I am convinced in the matter of the burying of the old etymology. On the other hand I am not wholly convinced by the new one. In paragraph 3.2.5.1 there is the suggestion of an original forensic sense of 'defending in a court of law, championing justice'. We must consider such etymologies in the full light of the cultural context and, if one is suggesting such an etymology for a Hebrew root or indeed a root found in other Semitic languages, one has to consider whether there is the evidence for the cultural situation in which you HAVE a court of law with a defender. I think there is not since, certainly right down to the middle of the second millennium and probably later, in the Mesopotamian field, justice was reached by the taking of evidence in a court of law; if no conclusive decision could be reached on that ground, it was not a matter of prosecuting and defending, the accused was then put to the oath and ultimately to the ordeal. Similarly in Israel we have at least the traditional evidence that Moses himself decided cases. In some instances we have evidence that this was given by divine decision but certainly not until after Moses would there have been any question of having a defender in a court of law so that this forensic process could be assumed. And I take it that the argument is that the root in question must have reached this kind of meaning well before this historical situation.

SAWYER: I would certainly accept what Professor Saggs has said. My argument

is based on linguistic evidence and one of the conclusions I came to at the end of this little study was that in fact historical, social and cultural evidence, if it is available, has in the last resort priority over the linguistic evidence; so I would accept this gratefully as an illustration of the conclusion I reached, if that is not too paradoxical.

FENTON: I did, of course, take the point that Professor Saggs made about the difficulty of establishing defendants in the law court. I had noticed that point in the paper but I did think that the forensic background of the term *hošia'* could stand up without reference to the specific thought of a counsel for the defence, which seems a bit modernistic. However, even here one ought perhaps to add a footnote to the effect that the late *bassar* of Syriac, which is I believe used to some extent in this manner, seems to have a reflex in an earlier context—if Professor Driver's reconstruction of part of the book of Job is correct then *mābasār* in Hebrew would appear to be 'someone who acts as a vindicator in the law court'. I have found this convincing and it would seem to me that, without thinking of a formal counsel for the defence in any modern sense, perhaps one of the witnesses may have been specifically charged to produce evidence in favour of the accused.

ULLENDORFF: I accept what Professor Saggs said, though of course we have already got the concept of the accuser and the defender in the notion of *Satan*, which of course is precisely that in the book of Job. So in the biblical context that idea was well known. At the same time I do not wish to suggest that Dr. Sawyer should retract his retraction, and I certainly do not accept what Dr. Fenton said that the idea of a defender is modernistic—in the inter-testamental period it is well known and in the Mishnaic period *ṣanêgôr* and *qāṭêgôr* are of course standard terms, so that for the idea so well-known from English law courts of a defender and an accuser there are terms attested for at least the last two and a half thousand years.

FENTON: My use of 'modernistic' was very relative.

SAGGS: I certainly accept that there was the idea of a defender in the first millennium. I can give examples myself of that from New Babylonian, definitely of a defender. There was a term for it. I am talking about going beyond the first millennium to the period when these terms would have to diverge in the different languages.

BARR: I think it shows how points of cultural history have to be looked at very carefully when we investigate linguistic relationships. I think that in general this paper shows that any etymological comparison between linguistic items in one language and another has to be modified by the consideration that in each language every word or unit forms part of a semantic field with the other units with which it is related, and the meanings it has in each language depends on the meanings of these other words and therefore it is not isolatedly connected with cognates elsewhere.

## LEXICOSTATISTICS AND THE INTERNAL DIVISIONS OF SEMITIC

CHAIM RABIN

The method of lexicostatistics, or as it is also called, glottochronology, was created by the American linguist Morris Swadesh, and is expounded by him in two articles: "Lexicostatistical dating of prehistoric ethnic contacts, with special reference to North American Indians and Eskimos" (1952) and, in a revised form, "Towards greater accuracy in lexicostatistic dating" (1955). It is based on the theory that items of the vocabulary of a language are replaced at a fixed rate. By observing how much of a list of basic words, common to all languages, is still the same as compared with an older stage of the same language, it should therefore be possible to estimate the time that has passed between the two stages, and similarly, by counting what percentage of that list is expressed by cognate words in two languages known to be related, the time at which they began to diverge from their common ancestor can be found. In principle, if there is enough data to fix a probable date of divergence, the rate of similar words could also be used in order to investigate whether two given languages are related or their lexical similarities are due to accident or borrowing, as the case may be. It is of course admitted that the rate of language change varies, but since lexicostatistics deals in thousands of years, periods of quick change and of conservatism should cancel each other out. The rate of change for the original basic list was worked out for a number of languages by Robert B. Lees (now Professor of Linguistics at Tel Aviv University) at 19.52% of words replaced, or, as it is more conveniently expressed, a retention rate of 80.48%. In his second study (1955), Swadesh selected out of the original list the sense-items which proved to be the best preserved in the languages tested, and with their help built up a smaller list of 100 items, for which the retention rate is 86%. We have used this later list except for the item 'yellow', as yellow and green are not distinguished in many Semitic languages and 'yellow' would therefore have been a repetition of 'green'. We replaced it by a new item, 'make'.

The question of the selection of items is a thorny one. While for measuring the time that elapsed between two *états de langue* a high retention rate is of advantage, items retained by all languages have no value in working out the relative positions of the different languages on the time-scale; neither have, for that matter, items for

which each of the languages tested has a different word. An improvement that could therefore be suggested is to remove from the list here used the items falling under the above two categories, and to replace them by others, which have a definite 'profile' of distribution. Since Swadesh (1955, 134 onwards) gives the persistence rate for all the items of his original larger list, a selection of additional items for Semitic could be made from those with a high persistence rate. The method of item substitution could also be used to remedy the lack of twelve items from the Ugaritic column. Apart from the labour involved in establishing correspondences for the new items (many of which would probably have to be rejected because they would prove to fall under one of the two categories mentioned above), the question of establishing a new retention rate has to be worked out. The present list has the added advantage of being comparable with lists made for languages outside Semitic.

The main problem, however, is the establishment of the word lists for each language. Quite apart from the fact that even for such basic words there may occasionally not be a clear lexical equivalent in a certain language (cf. the question of 'yellow' mentioned above), the Semitic languages we have used here are all literary languages. This means that for many sense-items they have a number of words, mostly differentiated by stylistic level. Swadesh insists, rightly, that in each case only one word should be considered, namely, the most commonly used one. We have no frequency lists for any of the languages studied, and must go by guess-work; besides, frequency in a certain group or type of literary text may have been quite different from frequency in common speech. The textual material in the different languages is not commensurate stylistically. We have therefore selected for inclusion in the word-list those words which are commonly used in prose, without deciding about frequency where we felt that the differences in frequency were not extreme, and have provided a Commentary, in which rarer words are given, and also cognates indicated where the difference of meaning was not considerable. This material will allow the reader to judge to what extent our inclusion or non-inclusion of any word in the basic list has been justified, and to set up, if he wishes, a list of his own. Of course I had to use my private judgment not only in this matter, but also in deciding which etymologies are certain enough to be given without comment, which required the qualifying 'probably', and which to omit altogether. We have included only material from the six languages listed. Clearly, the inclusion of material from other Aramaic dialects, for instance, would have brought in many more cognates with Hebrew, but would not have been relevant for establishing the divergence date for Syriac.

Another source of inaccuracy peculiar to our material is the ever-present possibility that the meaning given to a word included in the list may be wrong, or that the word (especially in the case of Akkadian) may be non-existent. The danger is particularly high for Ugaritic, but as is well known, there are divergent opinions even concerning the meaning of Biblical Hebrew words. With regard to Akkadian, I have made use of the *Chicago Assyrian Dictionary* (1956 ff.) and von Soden (1959 ff.) as far as these two go at present, and have had most valuable advice from Dr. A. Shafer, to whom

I wish to express my gratitude. For Syriac and Ethiopic, use has been made of the early Bible translations in order to establish which of the synonyms registered in the dictionaries were in use at the time for which our calculations are made (see below, p. 96) and what was their meaning.

	HEBREW	UGARITIC	SYRIAC	AKKADIAN	ARABIC	ETHIOPIO
(1) 'all'	kol	k l	kul	kalū	kull	kwēll
(2) 'ashes'	'ēpher	' m r	qeṭmā, rmū'ā	tumru, dikmēnu	rimād	ḥamad
(3) 'bark'	(qēlippāh)		qlāphthā	qulpu	qirfah	qērfat
(4) 'belly'	beṭen	k b d	karsā	karšu	baṭn	karš, kabd
(5) 'big'	gadhōl	g d l, r b, m i d	rabbā	rabū, ma'du	kabīr	'abiy
(6) 'bird'	šippor	' š r	šeppra	iššuru	'uṣfūr	'ōf
(7) 'bite'	nāšakh	n ṭ k	nkhath	našāku	'aḏḏa, kadama	nasaka
(8) 'black'	šāḥōr		ukkāmā	šalmu	'aswadu	šalīm
(9) 'blood'	dām	d m	dmā	dāmu	dam	dam
(10) 'bone'	'ešem	' ṣ m	garmā	ešemtu	'aẓm	'aḏm
(11) 'breast'	ḥāzeh		ḥadhyā	irtu	ṣadr	angēd'ā
(12) 'burn'	šāraph	š r p	auqedh	šarapu	ḥaraqqa	aw'ēya
(13) 'claw'	šippōren		ṭephra	šupru	ẓufr	šeṣr
(14) 'cloud'	'anān	' r p t, ḡ r p l	'nānā, 'arpellā	erpetu	ḡamāmah	damanā
(15) 'cold'	qar		qarrirā	kašū	bārid	qwarir
(16) 'come'	bā'	b w ', ' t w, m ḡ y	ethā, mṭā	alāku	'atā, jā'a	maš'a
(17) 'die'	mēth	m w t	mīth	mātu	māta	mōta
(18) 'dog'	kelebh	k l b	kalbā	kalbu	kalb	kalb
(19) 'drink'	šāthāh	š t y	eštī	šatū	šariba	sataya, šaraba
(20) 'dry'	yābheš	ḥ r b	yabbīšā	ablu	yābis	yēbūs
(21) 'ear'	'ōzen	u d n	edhnā	uznu	'uḏn	ēzn
(22) 'earth, soil'	'ādhāmāh	a r š	addamthā	eršetu, qaqqaru	turāb	mēdr
(23) 'eat'	'ākhal	' k l	ekhal	akālu	'akala	bal'a
(24) 'egg'	be(y)šāh		bēy'thā	pelū	baiḏah, kaikah	anqwaqēho
(25) 'eye'	'ayin	' n	'ainā	īnu	'ain	'ayn
(26) 'fat' (n.)	dešen		mešḥā	lipū, nāḥu	duhn	šēbh
(27) 'feather'	nōšāh		merṭā	nāšu	rīšah	šagwēr
(28) 'fire'	'ēš	i š t	nūrā	išātu	nār	ēšāt
(29) 'fish'	dāgh	d g	nūnā	nūnu	samak	'āšā
(30) 'fly' (v.)	'āph	' w p	praḥ	naprusu	ṭāra	sarara
(31) 'foot'	reghel	p ' n	reghlā, qāyemthā	šēpu	qadam, rijl	ēgr
(32) 'full'	mālē'	m l '	mlē	malū	mal'ān	mēlū'

(33) 'give'	nāthan	y t n	ya(h)bh, impf. nettel	nadānu	'a'ṭā, wahaba	wahaba
(34) 'good'	ṭōbh	ṭ b, d m q, m l ḥ	ṭābhā, šappirā	damqu	ṭayyib	šanāy, ḥēr
(35) 'green'	yārōq		yūrāqā	arqu	'aḥḍaru	ḥamalmīl
(36) 'hair'	šē'ār	š ' r	s'artha	šārtu	ša' r	šē'ert
(37) 'hand'	yādh	y d	idhā	qātu	yad	ēd
(38) 'head'	rō(')š	r i š	rēšā	rēšu	ra's	rē'ēs
(39) 'hear'	šāma'	š m '	šma'	šemū	sami'a	sam'a
(40) 'heart'	lēbh	l b	lebbā	libbu	qalb, fu'ād	lēbb
(41) 'horn'	qeren	q r n	qarnā	qarnu	qarn	qarn
(42) 'I'	'ānī, 'ānōkhī	a n, a n k	enā	anāku	'anā	ana
(43) 'kill'	hāragh	h r g	qṭal	dāku	qatala	qatala
(44) 'knee'	berekh	b r k	burkā	birku	rukbah	bērḵ
(45) 'know'	yādha'	y d '	idha'	idū	'alima, 'arafa	ayde'a
(46) 'leaf'	'āleh		ṭarpā	aru	waraqah	qwašl
(47) 'lie'	šākhabh	š k b	škhebh, rba'	rabāšu, šalalu	raqada, ḍaja'a	sakaba
(48) 'liver'	kābhēdh	k b d	kabhdā	kabittu	kabid	kabd
(49) 'long'	'ārōkh	a r k (v.)	arrikhā	arku	ṭawil	nawālḥ
(50) 'louse'	kinnām		qalmthā	kalmatu	qamlah	qwēmāl
(51) 'make'	'āsāh	b ' l	'bhadh	epēšu	'amila, šana'a	gabra
(52) 'man'	'iš	m t	gabhrā	awīlu	rajul	be'esī
(53) 'many'	rabbīm	m i d	rabbā	ma'du	kaṭīr	bēzūḥ
(54) 'meat'	bāšār	b š r	besrā	šeru	laḥm	šēgā
(55) 'moon'	yārēah	y r ḥ	sahrā	arḥu	qamar	warḥ
(56) 'mountain'	hār	ḡ r	ṭūrā	šadū	jabal	dabr
(57) 'mouth'	peh	p	pūmā	pū	fam	af
(58) 'name'	šēm	š m	šmā	šumu	ism	sēm
(59) 'neck'	šawwā(')r	ḥ l q ?	šaurā	kišādu	'unq	kēsād
(60) 'new'	ḥādhāš	ḥ d ṭ	ḥadhthā	eššu	ḥadīṭ, jadīd	ḥaddīs
(61) 'night'	lailāh	l l ?	lelyā	mūšu	lailah	lēlīt
(62) 'nose'	'aph	a p	nḥīrā	appu	'anf	anf
(63) 'not'	lō(')	l	lā	lā, ul	lā, mā	ī
(64) 'one'	'eḥadḥ	a ḥ d	ḥadh	ištēn	'aḥad	aḥadū
(65) 'person (human being)'	'ēnōš	bunušu	bar-(')nāšā	awīlu	'insān	ēgwāla- ēmmaḥēyāw
(66) 'rain'	gešem, māṭār	g š m, m ṭ r	šeghmē, meṭrā	zunnu	maṭar	zēnām
(67) 'red'	'ādhōm	a d m (v.)	sumqā	sāmu	'aḥmaru	qay(i)ḥ
(68) 'road'	derekh	n t b	urḥā	urḥu, ḥarrānu	sabīl, ṭariq	fēnōt
(69) 'root'	šōreš	š r š	šeršā	šuršu	'ašl	šarw
(70) 'round'	'āghōl		glīlā	kippatu (n.)	mustadīr	kēbūb
(71) 'sand'	ḥōl		ḥālā	baššu	raml	ḥōšā
(72) 'say'	'āmar	r g m	emar	qabū	qāla	bēhlā



(73) 'see'	rā'āh	' m r, ḥ d y, p h	ḥzā	amāru, naṭālu	ra'ā	rē'ēya
(74) 'seed'	zera'	d r ' (v.)	zar'ā	zēru	zar'	zar'
(75) 'sit'	yāšabh	y ṭ b	ithebh	ašābu	jalasa, qa'ada	nabara
(76) 'skin'	'ōr		meškā	mašku	jild, 'adīm	mā'es, anadā
(77) 'sleep'	yāšēn	y š n	dmekh	itūlu, šalālu	nāma	nōma
(78) 'small'	qāṭān	ṣ ḡ r, d q	z'ōrā	ṣeḥru	ṣaḡir	nē'ūs
(79) 'smoke'	'āšān	q ṭ r	tennānā, yaḥbūrā	qutru	duḥān, buḥār	tan, ṭis
(80) 'stand'	'āmadh		aqīm	izuzzu	qāma	qōma
(81) 'star'	kōkhābh	k b k b	kaukbhā	kakkabu	kaukab	kaukab
(82) 'stone'	'ebhen	a b n	kēphā	abnu	ḥajarah	ēbn
(83) 'sun'	šemeš	š p š	šemšā	šamšu	šams	ḡaḥāy
(84) 'swim'	šāḥāh		šḥā	naqlupu	sabaḥa	šabata
(85) 'tail'	zānābh	ḡ n b	dunbā	zibbatu	ḡanab	zanab
(86) 'that'	hahū(')		hau	ullū	ḡālika	zēkkū
(87) 'this'	hazzeḥ	h n d	dnā	annū	hāḡā	zē
(88) 'thou'	'attāh	a t	a(n)t	atta	'anta	anta
(89) 'tongue'	lāšōn	l š n	lešānā	lišānu	lisān	lēšān
(90) 'tooth'	šēn	š n t (pl.)	šennā	šinnu	sinn	sēnn
(91) 'tree'	'ēš	' š	ilānā	išu	šajarah	'ēḡ
(92) 'two'	šnayim	ṭ n	trē(y)n	šina	iṭnāni	kēl'ē
(93) 'walk'	hālahk	h l k	ezal	alāku	ḡahaba	ḡōra
(94) 'warm'	ḥām	ḥ m	ḥammīmā, šḥen (v.)	emmu, šaḥanu (v.)	ḥāmm, ḥārr, suḥn	mēwūq
(95) 'water'	mayim	m y m	māyyē	mū	mā'	māy
(96) 'we'	'ānaḥnū		ḥnan	nīnu	naḥnu	nēḥna
(97) 'what'	mah	m h	mā	mīnum	mā	mēnt
(98) 'white'	lābhān	l b n	ḥewār	pīšu	'abyaḡu	šā'ēdā
(99) 'who'	mī	m y	man	mannu	man	mannū
(100) 'woman'	'iššāh	a ṭ t	i(n)ttā	sinništu	imra'ah	bē'ēsīt

## COMMENTARY

(2) H. 'ēpher also 'dust'. S. *rmū'ā* is usually connected with Ar. *ramiḡa* 'to burn' and cognate words, but in view of U. 'mr may have an original '. The number of metatheses in this item is remarkable. Also E. *ḥamad* connects with Ar. *ḥdm* 'to be very hot', cf. *ḥadamah* and *ḥamadah* 'sound of blazing fire'.

(4) For U. *kbd* 'belly' cf. I Aqhat 116 etc. H. once *kārēš* Jer. 51:34; Ar. *kirš* 'stomach of a ruminant'.

(5) For *m'd* cf. H. *mē'ōdh* 'wealth; very', Ar. *imta'ada* 'to acquire wealth'. For E. 'abiy cf. H. 'ābhāh 'to be thick', Ar. 'aḡbā 'dense, tangled'.

(6) For E., cf. H. 'ōph 'birds'. Ar. seems a blend of \*'iṣṣuru and ṣippuru.

(7) Ar. *nakaṭa* 'to break, untwist'. For 'aḡḡa, cf. S. 'ṣṣ 'to compress (esp. the lips)'.

(8) For H., cf. S. *šhar* 'to be black', Ar. *šuhḥār* (š!) 'soot'. For Ak., E., cf. H. *šalmā-weth* (see lexx.), Ar. *ḡulm* 'darkness'.

- (10) For S., cf. H. *gerem* (see lexx.), Ar. *jirm* 'body' (of an animal—cf. H. 'āšāmōth = 'body').
- (11) Ak. *irtu* from \**iztu*. Ar. *ḥiḏā* 'front'. For E., cf. H. *neghedh* 'in front of, against'?
- (12) For S., cf. H. *yāqadh* (poet.), Ar. *waqada* 'to burn'. For Ar., cf. H. *ḥārakh*, S. *ḥrakh*, id.
- (14) H. also (poet.) 'ārābhōth, 'ārāphel.
- (15) For Ar., cf. *barad* in H., S., E. 'hail'.
- (16) For H. cf. *bw* in U., Ak., E. 'to enter', Ar. 'to return'. H. also 'āthāh (poet.). For U. *mgy*, S. *mṭā*, E., cf. H. *māšā* 'to find'; Ham.-Sem., cf. Cohen (1947), No. 474.
- (19) Possibly also U. *šrb*.
- (20) For U., cf. H. *ḥārēbh* 'to dry up; to be destroyed'; in Ak., Ar. 'to lie waste'. For Ak., cf. H. 'ābhēl 'to whither; to mourn'.
- (22) H. 'ereš 'earth, land', also often 'ground'. S. *medrā*, Ar. *madar* 'clod of earth'.
- (23) For E., cf. H., S., Ar. *bl* 'to swallow'. E. has *ēkl* (n.) 'food'.
- (24) Probably Ar. *kaikah* (rare) and E. represent the original word, cf. Cohen (1947), No. 195. *byḏ* is 'white' (cf. No. 98), Ak. *pelū* is 'yellow-red'.
- (26) For H., cf. Ar. *dasam* 'greasiness', for S., cf. H. etc. *mšh* 'to anoint'. For Ar. cf. S. *duhānā* 'fat, ointment'.
- (27) Ar. *nāṣiyah* 'forelock'.
- (28) S. *ešāthā* 'heat of fever'. *nwr* in other languages 'to shine'.
- (29) Ar. *nūn* 'large fish', is borrowed from Aramaic. The letter names (Phoenician) *nūn* and *samek* are supposed to mean 'fish'.
- (30) Ar. 'āfa 'to circle above' (of a bird). H. 'ephrōah, Ar. *farah* 'young bird'. Ar. *ṭair*, S. *ṭairā* 'birds', S. *aṭṭr* 'to cause to fly'. Ar. *sarra* 'to rejoice' (cf. Ar. *ṭāra min as-surūr* 'to be very happy').
- (31) Ar. also *rijl*. E. *ēgr* (also in Syrian Colloquial Ar.) is a metathesis of *rigl*, cf. Mandaean *ligrā*. H. poet. also *pa'am* 'foot', Ak. *pēmu* 'femur'. For Ak., cf. Mishnaic H. *šophī* 'caput femoris'.
- (33) H. *habh*, imp. 'give!', from \**yhb*.
- (34) S. also *ṭābhā*; Ak. *ṭābu* 'sweet-smelling'. Ar. also *malṭh*. For E., cf. Ar. *ḥair* 'better; good things', Ak. *ḥāru* 'to select'.
- (40) For Ar. *qalb*, cf. H. *qerebh*, Ak. *qirbu* 'inside of the body'. For Ar. *fu'ād*, cf. Ug. *pid*, said to mean 'mercy'.
- (43) For *hrg*, cf. S. *hragh* 'to meditate', Ar. *harija* 'to be excited', *haraja* 'to riot', (South-Arabian *hrg* 'to kill, take booty') E. *māhraka* 'to take booty and prisoners'. H. *qāṭal* late, and probably from Aramaic; Ak. *kattillu* 'wild (animal)'. For Ak., cf. H. *dwk*, Ar. *dāka* 'to crush, pound'.
- (45) For Ar. 'alima, cf. Ug. 'lm, perhaps 'learned' (Aistleitner); H. *ne'ēlam* 'to be unknown'.
- (46) The connection between H. and Ak. is not sure. Cf. also S. 'elwā 'leaf' (doubtful), Ar. 'aghlā 'to strip a vine of its leaves', *ghalā* 'to grow (plant)'. For S., cf. H.

(rare) *ṭarpēy* 'leaves' (cstr.), Ar. *ṭaraf* 'extremity'. For E., cf. Ar. *qaṣala* 'to give green fodder'.

(47) For S. *rbha* 'Ak. *rabāṣu*, cf. H. *rābhaṣ*, Ar. *rabaḍa* 'to crouch (of animals)'. For Ak. *ṣalālu*, cf. Ar. *zalla* 'to continue throughout a day or a night'. E. *nwlh*, in H., S., U., Ar. 'to rest, halt'. Ar. *nawwaḥa* 'to stretch something'(?), said of God shaping the earth.

(51) For U., cf. H. (poet.) *pā'al*, Ar. *fa'ala* 'to do', E. *mab'al*, *mā'bal* 'tool'. For S., cf. H. *'ābhadh* 'to work', U. *'bd* 'to serve', Ar. *'abada* 'to worship', E. *'abaṭa* 'to impose forced labour'. For Ak., cf. H. *hippēś* 'to seek', Ar. *ḥafaṣa fī* 'to apply oneself to', E. *ḥafaṣa* 'to seize, carry off'. For Ar. cf. H., S. *'ml* 'to do'; E. *ṣan'a* 'to be hard, strong', Hebrew *hiṣnia* 'to act cautiously or carefully'. For E., cf. next item in S.: E. *gabra* 'to do, make' is derived from E. *gabr* 'slave, servant', as S. *'bhadh* is related to S. *'abhdā* 'servant'.

(52) For U., cf. Hebrew poet. *mēthīm* pl. 'men', Ak. *mutu*, E. *mēt* 'husband'. For S., cf. H. poet. *gebher* '(young) man', Ak. *gabru* 'strong' (only lexica), Ar. *jabbār* 'giant, tyrant' (borrowed perhaps from Aramaic), E. *gabr* see no. 51. For Ak., cf. U. *ul* 'army', Ar. *'āl* 'tribe, family', *'āla* 'to be a chief', H. *'ēylēy* 'chiefs of'. For E., cf. Ar. *ba's* 'strength, courage'. For *mt*, cp. perhaps also Ar. *taim* 'slave'.

(53) For U., S., cf. H. *mē'ōdh* 'very'. For Ar., cf. H. *kāšēr* 'fit', Ak. *kušīru* 'success'.

(54) For Ak., cf. H. (rare) *šē'ēr* 'meat, flesh, relatives', Ar. *ṭa'r* 'blood revenge for relatives'.

(55) For S., cf. H. *ṣahārōnīm* 'crescent-shaped disks', Ar. *ṣahr* 'month', E. *ashērt* 'new moon'. For Ar., cf. E. *qamar* 'circle'. Ar. *ta'riḥ* 'date'.

(56) U. *hr* (doubtful). If U. from *\*zūr* = S., cf. Heb. *šūr* 'rock', Ar. *zīrr* 'sharp-edged stone'. For Ak., cf. H. *šādheh*, U. *šd* 'field'. For Ar., cf. H. *gēbhūl(āh)* 'territory, boundary'. For E. cf. H. *dōbher* 'pasture', S. *dabhrā* 'field', perhaps Ar. *dibār* 'irrigation channels'. Note that *ṭūrā* is also 'field' in Palestinian Aramaic dialects.

(59) Ak. also *unqu* 'neck', cf. S. *'eqqā* 'necklace', H. *'ānāq* 'necklace', E. *'anaqa* 'to put something round one's neck'.

(61) For Ak., cf. Ar. *masā* 'evening'.

(62) For S., cf. H. *nēḥirayim*, Ak. *naḥēru* 'nostril', Ar. *nuḥrah* 'tip of nose'.

(64) For Ak., cf. H. *'aštēy* *'ešrēh*, U. *'št* *'šr* 'eleven'.

(65) U. probably *bn-nš* 'son of man' = S., but possibly *\*bn-'š* (cf. 52). The E. means 'son of the mother of the living'.

(66) For Ak., E., cp. H. poet. *zerem* 'rain' (and see No. 92).

(68) H. *derekh* is in my view related to Ar. *ṭarīq* by de-pharyngalization near *r*, cf. Ak. *durgu*, *duruku* 'far-away region' (CAD). For U., cp. H. poet. *nēthīb(āh)* 'road'. For S. and Ak. (*urḥu*), cp. H. poet. *'ōrah* 'road', and probably E. *marḥa* (*marḥa*?) 'to show the way' (secondary root). For Ar. *sabīl*, cf. H. poet., S. rare *šēbhīl(ā)*.

(70) For S., cp. H. *gll*, E. *angargara* 'to roll', Ar. *'ajāla* 'to wheel a thing around'. For Ak. and E., cp. S. *kpp* 'to bend', H. *kāphaph* 'to incline', Ar. *kifāf* 'rim'.

(71) For H., S., cf. Ar. *ḥāl* 'hot ashes, black mud'. For E., cf. H. *ḥāṣāṣ*, S. *ḥṣāṣā* (Nestor. *ḥāṣṣā*), Ak. *ḥiṣṣu* 'gravel', Ar. *ḥaṣā(n)* 'pebbles'.

(72) For H., S., cp. U. *amr* 'a wish', Ar. *'amara* 'to command'. For U., cp. Ak. *ragāmu* 'to shout, howl, accuse', Ar. *rajama* 'to curse, revile; speak vaguely', E. *ragama* 'to curse', perhaps H. *rgn* 'to murmur', E. *angwargwara* 'to murmur, complain' and/or *nagara* 'to speak'. For Ak., cp. H. *qbb* 'to curse', Ar. *qabqaba* 'to bray, roar'. For Ar., cp. Ak. *qālu* 'to shout', and the nouns H. *qōl*, S. *qālā*. E. *qāl* 'voice, sound, what is said', Ak. *qūlu* 'a cry, complaint'. For E., cp. Ar. *bahala* 'to curse', perhaps also Ak. *apālu* 'to answer'.

(73) For U., Ak. *'mr*, cp. E. *ammara* 'to show', *a'mara* 'to know'. For S., cp. H. poet. *ḥāzāh* 'see', Ar. *ḥazā* (*ḥzw*) 'to guess', *'aḥzā* 'to be prominent; to know', the relation with U. *ḥdy* is not clear. For Ak., perhaps Ar. *ṭalla* 'to look down from above'.

(75) See Rabin (1971).

(76) For H., cp. perhaps Ar. *'aurah* 'pudenda' (relation to H. *'erwāh*, id., and H., S. Ak., Ar. *'ry* 'to be naked'?). For S., Ak., cp. H. *mešekh* 'skin bag', Ar. *mask* 'freshly-flayed skin'. For Ar. *jild*, cp. H. (once in Job) *gēledh*, S. *geldā*, late Ak. (from Aram.?) 'skin'.

(77) For H., U., cp. Ar. *wasina* 'to be in deep slumber', and the noun S. *šenthā*, Ak. *šittu*, Ar. *sinah* 'sleep'. For Ar., E., cp. S. *nām* 'to be in deep slumber', H. poet. *nām* 'to sleep', Ak. *munāttu*, pl. *munāmātu* 'slumber in the morning'.

(78) For H., cp. S. (rare) *qaṭṭinā* 'small', Ak. *qatnu* 'thin', *quttennu* 'small', Ar. *qaṭṭin* 'slave', E. *qaṭṭin* 'thin, small'. For U., S., Ak., Ar., cp. H. *zē'ēyr* 'a little', *ša'ir* 'young'. For U. *dq*, cp. H. *daq* 'thin', S. *daqqiqā* 'small', Ak. *daqqu* 'small; young child', Ar. *diqq* 'fine', *daqiq* 'thin', E. *daqqiq* 'small, thin; young child(ren)'. E. seems to be connected with roots *'nš* (H., Ak.), *nšš* (S.) meaning 'to be weak'.

(79) For H., cp. Ar. (rare) *'aṭan* 'smoke', which in S. would become *\*'tan* 'it smoked', which appears, with loss of *'*, and restructured as geminate root, as *tan*, from which S. *tennānā*; hence E. *tan* must be borrowed from S. For U., Ak., cp. H. (rare) *qīṭōr* 'smoke', and H. *qētōreth*, E. *qētārē* 'incense', Ar. *qutār* 'smoke of aloes-wood (and of cooked meat)'.

(80) For H., cp. Ak. *emēdu* 'to approach, lean on, stand near something', Ar. *'amada ilā* 'to go towards', *'ammada* 'to stop (a stream)'. H. *qām* is 'to get up'.

(82) S. also (rare) *abhnā*. For S., cp. H. *kēphim* 'rocks' (from Aramaic?), Ak. *kāpu* 'rock', perhaps Ar. *kūfah* 'mound of sand'.

(84) For H., S., cp. Ak. *šahū* 'to wade in the mud', Ar. *šahḥa* 'to make water'.

(91) Ar. *'aḏā* 'thicket of trees', *'iḏah(un)* 'any thorny tree'. For S., cp. H. *'ēlāh*, *'ēlōn*, names of definite trees or of any large tree, Ar. *'alā*, 'a certain green-leaved and bitter fruit tree'.

(92) To S.: a correspondence *n/r* also in common Sem. *binu*: S. *brā* 'son', H. *zerem*: *zēnām* (above, no. 66), H. *zārah*: S. *dnah* 'to rise, shine (sun)', H. *nāṭaš*: Targumic

Aramaic *rēṭaš* 'to abandon'. For E., cp. U. *klāt*, Ak. *kilallān*, Ar. *kilā* 'both', H. *kil'ayim* 'of two kinds'.

(93) For H., S., Ak., cp. S. *hallekh* 'to go away', Ar. *tahallaka* 'to swagger gracefully', *halaka* 'to perish, die', perhaps E. *hagwēla* 'to perish'. For S., cp. Ar. 'azala 'to go away', H. 'āzal 'to disappear'. For E., cp. S. *hār* 'to be near', *aḥīr* 'to start out on a way', Ar. *hāra* 'to return, to pass', perhaps also Ak. *ḥarrānu* 'road' (No. 68).

(98) For H., U., cp. Ar. *laban* 'milk'. For S., cp. Ar. *iḥwarra* 'to be intensely white'. For Ak., Ar., cf. 'egg' (No. 24).

(100) For H., U., S., cp. Ak. *aššatu* 'wife', Ar. 'unṭā 'female'.

## TABLE OF AGREEMENTS

In the following table, + means that the two languages of that column have cognate words in the basic list, — that they have words of different etymological origin. In the case of pronouns, this also refers to cognate and different formative elements. The table takes no account of cognates mentioned in the commentary. If there is no entry, the word for that semantic item is unknown in one of the two languages (this is always Ugaritic).

Abbreviations: Ak(kadian), Ar(abic), E(thiopic), H(ebrew), S(yriac), U(garitic).

	H U	H S	H Ak	H Ar	H E	U S	U Ak	U Ar	U E	S Ak	S Ar	S E	Ak Ar	Ak E	Ar E	
(1)	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	all
(2)	—	—	—	—	—	+	—	—	—	+	—	—	+	—	—	ashes
(3)			+	+	+					+	+	+	+	+	+	bark
(4)	—	—	—	+	—	—	—	—	+	+	—	+	—	+	—	belly
(5)	+	—	—	—	—	+	+	—	—	+	—	—	—	—	—	big
(6)	—	+	—	+	—	—	+	+	—	+	—	—	+	—	—	bird
(7)	+	+	+	—	+	+	+	—	+	+	—	+	—	+	—	bite
(8)		—	—	—	—					—	—	—	—	+	—	black
(9)	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	blood
(10)	+	—	+	+	+	—	+	+	+	—	—	—	+	+	+	bone
(11)			+	+	—	—				+	—	—	—	—	—	breast
(12)	+	—	+	—	—	—	+	—	—	—	—	—	—	—	—	burn
(13)			+	+	+					+	+	+	+	+	+	claw
(14)	—	+	—	+	—	+	+	—	—	—	+	—	—	—	—	cloud
(15)			+	—	—	+				—	—	+	—	—	—	cold
(16)	+	—	—	—	—	+	—	+	+	—	+	+	—	—	—	come
(17)	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	die
(18)	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	dog
(19)	+	+	+	—	+	+	+	—	—	+	+	—	—	+	+	drink
(20)	—	+	—	+	+	—	—	—	—	—	+	+	—	—	+	dry

(21)	+	+	+	+	+	+	+	+	+	+	+	+	+	+	ear
(22)	—	+	—	—	—	—	+	—	—	—	—	—	—	—	earth
(23)	+	+	+	+	—	+	+	+	—	+	+	—	+	—	eat
(24)		+	—	+	—					—	+	—	—	—	egg
(25)	+	+	+	+	+	+	+	+	+	+	+	+	+	+	eye
(26)		—	—	—	—					—	—	—	—	—	fat
(27)		—	+	—	—					—	—	—	—	—	feather
(28)	+	—	+	—	+	—	+	—	+	—	+	—	—	+	fire
(29)	+	—	—	—	—	—	—	—	—	+	—	—	—	—	fish
(30)	+	—	—	—	—	—	—	—	—	—	—	—	—	—	fly
(31)	—	+	—	+	+	—	—	—	—	—	+	+	—	—	foot
(32)	+	+	+	+	+	+	+	+	+	+	+	+	+	+	full
(33)	+	+	+	—	—	+	+	—	—	+	+	+	—	—	give
(34)	+	+	—	+	—	+	+	+	—	—	+	—	—	—	good
(35)		+	+	—	—					+	—	—	—	—	green
(36)	+	+	+	+	+	+	+	+	+	+	+	+	+	+	hair
(37)	+	+	—	+	+	+	—	+	+	—	+	+	—	—	hand
(38)	+	+	+	+	+	+	+	+	+	+	+	+	+	+	head
(39)	+	+	+	+	+	+	+	+	+	+	+	+	+	+	hear
(40)	+	+	+	—	+	+	+	—	+	+	—	+	—	+	heart
(41)	+	+	+	+	+	+	+	+	+	+	+	+	+	+	horn
(42)	+	+	+	—	+	+	+	+	+	—	+	+	—	—	I
(43)	+	—	—	—	—	—	—	—	—	—	+	+	—	—	kill
(44)	+	+	+	+	+	+	+	+	+	+	+	+	+	+	knee
(45)	+	+	+	—	+	+	+	—	+	+	—	+	—	+	know
(46)		—	+	—	—					—	—	—	—	—	leaf
(47)	+	+	—	—	+	+	—	—	+	+	—	+	—	—	lie
(48)	+	+	+	+	+	+	+	+	+	+	+	+	+	+	liver
(49)	+	+	+	—	—	+	+	—	—	+	—	—	—	—	long
(50)		+	+	+	+					+	+	+	+	+	louse
(51)	—	—	—	—	—	—	—	—	—	—	—	—	—	—	make
(52)	—	—	—	—	—	—	—	—	—	—	—	—	—	—	man
(53)	—	+	—	—	—	—	+	—	—	—	—	—	—	—	many
(54)	+	+	—	—	—	+	—	—	—	—	—	—	—	—	meat
(55)	+	—	+	—	+	—	+	—	+	—	—	—	—	—	moon
(56)	—	—	—	—	—	+	—	—	—	—	—	—	—	—	mountain
(57)	+	+	+	+	+	+	+	+	+	+	+	+	+	+	mouth
(58)	+	+	+	+	+	+	+	+	+	+	+	+	+	+	name
(59)	—	+	—	—	—	—	—	—	—	—	—	—	—	+	neck
(60)	+	+	+	+	+	+	+	+	+	+	+	+	+	+	new
(61)	+	+	—	+	+	+	—	+	+	—	+	+	—	—	night
(62)	+	—	+	+	+	—	+	+	+	—	—	—	+	+	nose
(63)	+	+	+	+	—	+	+	+	—	+	+	—	+	—	not
(64)	+	+	—	+	+	+	—	+	+	—	+	+	—	—	one
(65)	+	+	—	+	—	+	—	+	—	—	+	—	—	—	person
(66)	+	+	—	+	—	+	—	+	—	—	+	—	—	+	rain
(67)	+	—	—	—	—	—	—	—	—	—	—	—	—	—	red
(68)	—	—	—	+	—	—	—	—	—	+	—	—	—	—	road
(69)	+	+	+	—	—	+	+	—	—	+	—	—	—	—	root
(70)		—	—	—	—					—	—	—	—	+	round

(71)		+	—	—	—					—	—	—	—	—	—	sand
(72)	—	+	—	—	—	—	—	—	—	—	—	—	—	—	—	say
(73)	—	—	—	+	+	+	+	—	—	—	—	—	—	—	+	see
(74)	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	seed
(75)	+	+	+	—	—	+	+	—	—	+	—	—	—	—	—	sit
(76)		—	—	—	—					+	—	—	—	—	—	skin
(77)	+	—	—	—	—	—	—	—	—	—	—	—	—	—	+	sleep
(78)	—	—	—	—	—	+	+	+	—	+	+	—	+	—	—	small
(79)	—	+	—	—	+	—	+	—	—	—	+	+	—	—	—	smoke
(80)		—	—	—	—					—	+	+	—	—	+	stand
(81)	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	star
(82)	+	—	+	—	+	—	+	—	+	—	—	—	—	+	—	stone
(83)	+	+	+	+	—	+	+	+	—	+	+	—	+	—	—	sun
(84)		+	—	—	—					—	—	—	—	—	—	swim
(85)	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	tail
(86)		+	—	—	—					—	—	—	—	—	+	that
(87)	+	—	—	+	—	—	—	+	—	—	—	—	—	—	—	this
(88)	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	thou
(89)	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	tongue
(90)	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	tooth
(91)	+	—	+	—	+	—	+	—	+	—	—	—	—	+	—	tree
(92)	+	+	+	+	—	+	+	+	—	+	+	—	+	—	—	two
(93)	+	—	+	—	—	—	+	—	—	—	—	—	—	—	—	walk
(94)	+	+	+	+	—	+	+	+	—	+	+	—	+	—	—	warm
(95)	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	water
(96)		+	+	+	+					+	+	+	+	+	+	we
(97)	+	—	—	—	—	—	—	—	—	—	+	—	—	+	—	what
(98)	+	—	—	—	—	—	—	—	—	—	—	—	—	+	—	white
(99)	+	—	—	—	—	—	—	—	—	+	+	+	+	+	+	who
(100)	+	+	—	—	—	+	—	—	—	—	—	—	—	—	—	woman
	65	66	53	50	47	53	54	41	39	51	54	45	39	43	44	TOTAL

We can now proceed to calculate  $t_1$ , that is the time which elapsed between the date at which any one of the above 15 pairs of languages still formed one single language and were just beginning to diverge and the date at which we observe the older one of the two languages;  $t_2$  is the corresponding distance for the younger language. The list following the table of calculations gives the dates chosen and the reasons for the choice. The difference between  $t_1$  and  $t_2$  is called  $d$ .

To do this, we use  $C$  (Cognate Percentage), that is the percentage of words in the above list which are cognate in both languages and can therefore be assumed to have been retained from the time when both were one language. For effects of contact and borrowing, see below, p. 99.

For the list here used (except for the item 'make'), Swadesh (1955, 127) has found that the standard retention rate  $r$  is 86% for each thousand years, that is to say that after 1,000 years have elapsed, 86 words will still be cognate, after 2,000 years 74, and so on, with the words used in the original undivided language. This standard retention rate refers to the development of one language, and can be used to calculate

the time that elapsed between an earlier and a later stage of a language if the earlier stage is known and its vocabulary can be established. In the case of two languages diverging from an unknown common ancestor, a more complicated formula is needed (Swadesh 1955, 123), using this standard rate  $r$ . This formula is :

$$\frac{\log C}{\log r} - d \quad \text{or} \quad 2t_1 + d = \log C : \log r.$$

The result of this calculation gives us the number of years which elapsed between the start of the process of divergence of the two languages and the date of the older language, or in other words, if added to the date we have fixed for the older language it gives us the year B.C. at which the two languages were one.

Clearly, because of the smallness of the sample, there is fair margin of error, so that language pairs whose calculated dates of common origin lie near each other, may actually be assumed to have all diverged from a common ancestor at the same time. A source of error inherent in the nature of our material is that we cannot really be sure that all the words given for each language were in fact in common use in the year we have fixed here: not only did we have to take them sometimes from somewhat earlier or later sources, but they appear in literary documents which may have retained archaic usages, and in some cases words may have been replaced in the texts that have come down to us.

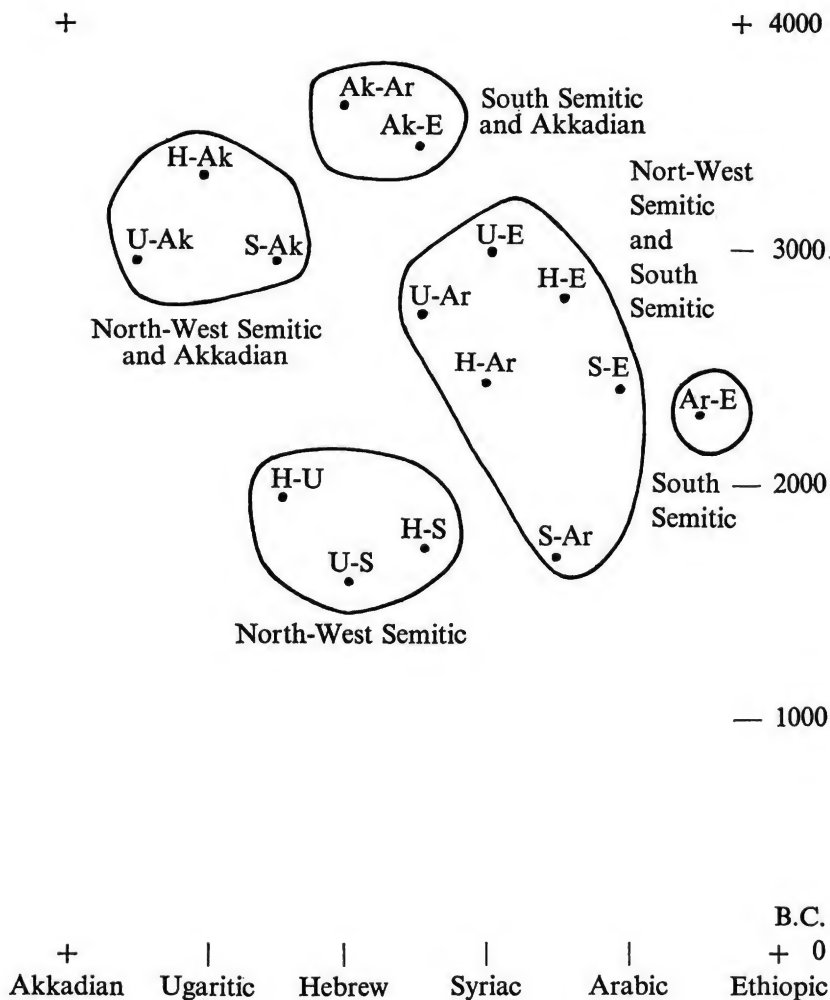
	$d$	$C$	$\frac{\log C}{\log r}$	$\frac{\log C}{\log r} - d$ 2	measured from	date of divergence
Hebrew-Ugaritic	600	0,79*	1563	480	Ugaritic	1900 B.C.
Hebrew-Syriac	1000	0,66	2771	880	Hebrew	1700
Hebrew-Akkadian	700	0,53	4209	1755	Akkadian	3250
Hebrew-Arabic	1400	0,50	4600	1600	Hebrew	2400
Hebrew-Ethiopic	1100	0,47	5000	1950	Hebrew	2750
Ugaritic-Syriac	1600	0,65	1940	170	Ugaritic	1575
Ugaritic-Akkadian	100	0,66	2771	1335	Akkadian	2900
Ugaritic-Arabic	2000	0,50	4600	1300	Ugaritic	2700
Ugaritic-Ethiopic	1700	0,48	4837	1568	Ugaritic	2950
Syriac-Akkadian	1700	0,51	4464	1382	Akkadian	2900
Syriac-Arabic	400	0,54	4085	1842	Syriac	1650
Syriac-Ethiopic	100	0,45	5295	2597	Syriac	2400
Akkadian-Arabic	2100	0,39	6243	2071	Akkadian	3575
Akkadian-Ethiopic	1800	0,43	5595	1897	Akkadian	3400
Arabic-Ethiopic	300	0,44	5444	2572	Ethiopic	2275

\* As Ugaritic has only 82 words in the list (the Ugaritic equivalents of the others being as yet unknown), the absolute totals for any pair including Ugaritic have been adjusted as percentages of 82.



Dates of languages used for calculating  $d$ 

Hebrew	800 B.C.	(Middle date of First Temple period)
Ugaritic	1400 B.C.	
Syriac	200 A.D.	(Peshitta)
Akkadian	1500 B.C.	(Old Babylonian)
Arabic	600 A.D.	(Pre-Islamic poetry and Qur'ān)
Ethiopic	300 A.D.	(Bible translation)



The dots lie horizontally half-way between the two languages they unite. Their vertical coordinate is the year (exact to 25) calculated as the date at which the two languages divided in their development.

The above table gives in graphic form the results for all the 15 pairs of languages. It will be seen immediately that some of the results agree with the assumptions of the family-tree theory of Semitic: the dates at which the North-West Semitic languages divided lie very low, and would fit the idea that around 2000 B.C. North-West Semitic was still one single language. The dates joining North-West Semitic to Akkadian lie significantly higher. In both cases the points lie close enough (within a range of 350 years) to suggest that the difference is due to the margin of error to be expected with small samples, and that in fact the separation of the languages from each other took place at one and the same time. This would mean that North-West Semitic (Hebrew and Syriac) separated simultaneously from Akkadian, being still a single North-West Semitic language. The conclusion is more surprising with the North-West Semitic languages themselves, as it would imply that Hebrew separated from Ugaritic about the same time it separated from Syriac (if anything, earlier), so that our table gives no support to the assumption of a proto-Canaanite including Ugaritic.

On the other hand our table is not in keeping with the view that West Semitic constitutes a branch jointly diverging from Akkadian. Both South Semitic languages here listed diverge from Akkadian at an earlier date than the North-West Semitic languages. The cluster of North-West Semitic-South Semitic links (except for S-Ar) lies significantly higher than the North-West Semitic cluster: if we are to believe the table, then the separation of North-West Semitic from South Semitic took place at the same time or very soon after the division of North-West Semitic from Akkadian. The cluster itself for North-West Semitic-South Semitic is far more widespread than any of the others: 1300 years, and even without the oddly low Syriac-Arabic date, 550 years. Finally, the link of Arabic and Ethiopic lies well before 2000, a date which seems to be in contradiction with all we know about the history of the Arabian peninsula. Note that according to our sample of words, Arabic differs much more from Ethiopic than from Syriac, and practically as much as Syriac does from Ethiopic. To this we may add the observation that a very high percentage of the agreements between Arabic and Ethiopic (30, = 63%) concerns cases where all of the languages listed agree. While placing the two South Semitic languages in a special relation to the other two branches, our results seem to speak against the assumption of a proto-South Semitic.

In this connection, another curious fact can be deduced from the basic list. Arabic has 53 words not found (at all or in this meaning) in any of the other languages (this includes in some cases two Arabic words under one number), and Ethiopic 40. In 30 of the 100 items, both Arabic and Ethiopic have words not found in any other language, but also differ from each other. They agree against the rest only in three cases: (19) *šariba*, (24) *kaikah*, (77) *nāma*. All three of these are weak evidence: *šariba* is suspect as a borrowing from Arabic; *kaikah* is a rare word, which perhaps should not have been included at all; *nāma* has cognates in other languages with a rather small difference of meaning. (Exclusion of *šariba* and *kaikah* would have raised the date of divergence for Ar-E to 2425 B.C., right into the range of North-

West Semitic-South Semitic links). For comparison, Akkadian has 33 words not found in any of the other languages here listed, Hebrew has 9, and there are 4 cases where both Akkadian and Hebrew differ, each in its own way, from all the rest. The separateness of the South Semitic vocabulary, and especially of the Arabic one, generally passes unnoticed because one looks at the dictionary as a whole, including all the rare and out-of-the-way words, and because Arabic in particular serves so much as a reservoir for the etymologist. It is one of the advantages of lexicostatistics to bring out such hidden features.

For the separateness of Ethiopic, one is likely to look for a cause in Cushitic influence. No doubt some of the words are Cushitic. But what can account for the separateness of the Arabic vocabulary in this basic range, which contrasts so oddly with its conservative phonology and what is generally considered to be a very conservative morphology? And lastly, how does the separateness of Arabic fit in with the large percentage of common vocabulary with Syriac, expressed in the low date of divergence? A search for possible loans from Syriac (Aramaic) into Arabic reveals not a single likely case; the words common to Syriac and Arabic, but not found in Hebrew or Ugaritic, are (28) *nūrā/nār*, (79) *yaḥbūrā/buḥār*, (80) *aqīm/qāma* (also E), (97) *mā*, (99) *man*. All this suggests the need for a further investigation of the more fundamental vocabulary of Arabic in order to establish more clearly its position within the Semitic family.

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#### DISCUSSION

BARR: I have to give apologies from Professor Rabin who was prevented at the last moment from coming. I think that one or two of our papers make the statement that no lexicostatistical or glottochronological studies in the Semitic field have been done, but here we have the demonstration that this is not entirely the case. Professor Rabin himself is not here but the work he has done is fairly clear in its type. It is the setting up of a list of terms for comparison between six groups of Semitic languages

and the attempt to derive from this on a mathematical model the probable dates for separation of the various language groups.

LESLAU: When it comes to glottochronology there are, I would say, two basic problems. One, the use of the same basic hundred words, whichever they may be, from one culture to another. Since the lexicon is culture-bound I seriously have my doubts whether one can use the lexicon of American Indian, let us say just to take an example, for the Semitic civilization. So there is a fallacy of choosing a basic hundred words for comparison and particularly for an important problem such as glottochronology and draw conclusions on this basis. Secondly, Rabin goes a little further in a way, but this is also a fallacy, by using etymology. It is true he does it only in his footnotes but he uses it to quite a considerable extent so as to stretch these basic hundred words. I remember for instance that for 'drink' he uses two words for Ethiopic, the root *s-t-y-* and *š-r-b-*. Now one has to make a decision. I think that *š-r-b-* is a loan-word and consequently it should not be used in this comparison. Now since Rabin is not here I will not enter into discussion of etymologies. There are a few which I do not completely agree with but this is a question of detail. But basically, to sum up my points, no particular hundred words should be used in a dogmatic way for glottochronology for all cultures, and etymologies should not be used for this purpose.

BARR: Just to clarify: are you arguing that you cannot have any hundred words for Semitic or that it must be a different hundred words from the ones you use for the American Indian languages? If you have a different hundred words for Semitic this would be all right?

LESLAU: Well, this I am not discussing. I do not know whether one hundred are really sufficient, but definitely one should not use the same one hundred words for American Indian and for Semitic.

KAYE: It is very interesting that lexicostatistics or glottochronology has not really been applied to Semitic and I think we shall all be thankful for that position. The Indianists have given it up, the anthropologists have given it up, ever since really a paper came out in *Language* by Douglas Chretien in 1962 in which he proved beyond the shadow of a doubt to everyone I think concerned that the mathematical models behind the approach are just entirely wrong. Two interesting things were brought up by Rabin in this paper from my point of view. First, the hundred word list is being used against so-called proto-Canaanite which includes Ugaritic. I am about the only one I think that still believes in Götze's 1943 paper in *Language* called "Is Ugaritic a Canaanite dialect?" in which he said: "No!". Now probably all Semitists specializing in North-West Semitic would disagree with that, but this lexicostatistical information still goes hand in hand with Götze's classic paper.

The other interesting point is that Rabin also argues against a proto-South Semitic, which is probably not the accepted view and what he is really saying goes hand in hand with a paper that will come up later for discussion, namely Robert Hetzron's paper on the postulation of a Central Semitic and I for one would be very interested in hearing other people's reactions to this doing away with proto-South Semitic and postulating a Central Semitic. I think there are many good reasons for doing this.

BENDER: I would like to make two comments. One is that what Professor Leslau said I certainly agree with, and it raises a problem. If you have a separate hundred word list for Semitic and another one for Indo-European and another one for Cushitic and so on, then you have no comparison. That is, the rate constant which was calculated as supposedly a universal would no longer apply because the items that you are using would actually differ from case to case. Secondly, I would like to reply to this last remark about the paper of Chretien, which I commented upon before in a very obscure place which I guess has escaped notice, namely in the *Journal of Ethiopian Studies*, volume 4, No. 1. Although basically I accept Chretien's conclusions I think he overstated his case. Without repeating my arguments here in detail I will merely say that I think that his objections do not invalidate results providing that one does not exceed time depths of say three millennia. Now I am not saying by this that I am defending glottochronology. Personally I am one of those people also who have dropped the whole idea. I think however that the idea of using basic vocabulary and percentage figures of cognates, forgetting about what relationship this may or may not have with time of separation, still has some validity.

TYLOCH: I would like to come back to the doubts expressed by Professor Leslau. Although I found Professor Rabin's paper very inspiring and instructive, I also feel that his conclusions are in disagreement with what we know from history and archaeology. As is well known Hebrew has sometimes been called a *Mischsprache* with two constitutive parts, Aramaic and Canaanite. The picture given of the situation in Professor Rabin's paper, though different from what we have been accustomed to until now, would seem to suggest that the whole question must be investigated further.

HODGE: In 1965 I had the opportunity of talking for a few minutes on glottochronology with Morris Swadesh who made it up. I think that he would agree also with everything that has been said. That is he did not look at it as something where you would get exact dates and so forth. He entirely agreed with this business of word lists not being good for all languages of the world and also he felt that it was a device to be used with languages of which we knew very little as a general survey technique to start the ball rolling. I do not know really whether he would have done it except just to see what came out with languages that we know as much about as we do with Semitic.

SAGGS: I hardly like to make a criticism in the absence of Dr. Rabin to defend himself. But what I notice is the arbitrary nature of some of the selections of words and I have gone very rapidly in the last few minutes through the Akkadian list and noticed that, for example, for No. 2 'ashes' two words are given, and yet *apru* or *epru* which is not only the etymological but also the semantic equivalent in some cases of Hebrew *'ēpher* is omitted; for No. 12 *šarapu* is given, chosen arbitrarily from a list of about four or five verbs which mean 'to burn'; No. 16 *bā* 'to come' has of course in Hebrew a more specific sense just as the same root has in Akkadian: 'to come along' rather than 'to come in'; No. 34 'good': for Akkadian *damqu* is given arbitrarily rather than *ṭābu* which is equally or more common; No. 64 'one': *ištēn* is given rather than the cognate *ēdu* which is equally or more common; No. 80 'stand': *izuzzu*, which should be *uzuzzu*, is given rather than *emēdu* with the same meaning, and there are one or two more one might mention.

## GENETIC CLASSIFICATION AND ETHIOPIAN SEMITIC

R. HETZRON

### 1. METHODOLOGICAL PROBLEMS

#### 1.1. *The rise of new languages*

To explain the formation of new languages out of an ancestor tongue, there have been two concurrent theories: (1) the tree theory (*Stammbaumtheorie*), which assumes that languages split into branches with each branch becoming an independent unit, and (2) the wave theory (*Wellentheorie*), which claims that linguistic changes start out from certain geographical points, spreading in a circle around them, and since different points may be the sources of various changes over the same territory, the lines of the resulting network of isoglosses crosscut each other. Even though phenomena that have spread in such waves are abundantly attested, let us note that the second theory, in order to gain the same explanatory power as the first one, has to be combined with the assumption that the geographical expansion of the ancestor tongue hardly affected its homogeneity so that the waves arose in an area which, prior to the effect of the waves, was basically uniform. Such a situation is possible, but is very rare.<sup>1</sup> Furthermore, the wave theory can only account for those instances in which territorial and cultural continuity is being maintained and cannot deal with separation by migration or political isolation. Thus, whereas the wave theory succeeds in explaining the rise of many isoglosses, it rarely explains the rise of new languages. On the other hand, a rigid conception of the tree theory may create the false impression that daughter-languages are rectilinear descendants of an ancestor tongue in the same way that children come from parents. It is clear that languages are not necessarily insulated entities, bundles of well-determined features and phenomena which draw sharp limits between each idiom, but that they

<sup>1</sup> Such a situation is posited for Semitic by Rabin (1963). His arguments are plausible in the sense that it is improbable that the actual division into languages results from different migrations, but rather represents the outcome of diversification of dialects which were already on the spot. On the other hand, one should not infer from this that all the branches of Semitic should be coordinate with each other and no intermediary branchings can be reconstructed.

allow for transition and graduality, for the taking over of features from neighbours at any moment of their existence, features that may affect the whole or any portion of the language area.

Nonetheless, most people will agree that there are such things as 'languages', with boundaries beyond which even closely related idioms must be considered to belong to 'another language'. Quite often these boundaries are identified with existing political borders. The frequent lack of adequate scientific criteria for establishing a sharp boundary between two adjacent closely related languages is indicative of the potentialities for the creation of new languages over the transitional border territory. Only the future will tell whether a clear limit between the two is going to be drawn (which is probable if there is a political boundary in the middle), or whether a new third language will be created between the original two. It is naturally risky to make predictions about the future, but when dealing with the past, where we know the course that later developments took, we can look back into it and try to reconstruct what really happened, what kind of discernible boundaries were established that led to the rise of distinct more sharply delimited languages. Such boundaries should be defined in terms of features, isoglosses, rather than on a purely geographical basis (which, in most instances, is impossible). With the proper historical perspective we can also see that really embarrassingly indefinite borderlines are only found between languages that are the closest relatives, the separation of which was the most recent split on their way to independence. For a further stage of branching to be entered limits *MUST* be drawn in one way or another. This means that the transitional quality of features must gradually disappear and at a certain point an abrupt difference will be found. Transition dialects may thus be disregarded as temporary phenomena, characterizing only one stage in development, when further branchings are still *in statu nascendi*. Consequently, when dealing with historical linguistics, our work is much easier. We are in the privileged position of handling past developments the outcomes of which are known to us. Therefore, we are much better placed as a result of the distance between past events and our times than if we were to put ourselves in the position of a contemporary observer.

All this shows that, while the wave theory does not provide any explanation for the rise of new languages in general because of its claim for uniqueness, the tree theory does not explain anything at all and is only the graphic representation of a divisionary development the criteria for which remain to be established.

The primary condition for the rise of a new language is the establishment of a cultural and/or political unit covering only part of the domain of the ancestor tongue. Such a unit may either be created by physical separation, the displacement of a group to a new territory where they have no further contact with the other representatives of the ancestor tongue,<sup>2</sup> or by the rise of new political powers or

<sup>2</sup> Physical separation does not necessarily entail creation of new languages. The natural tendency for divergence may be kept to a minimum when the original standards of the old language are preserved through education, or when the contact with the 'old country' is constantly kept alive.



cultural centres that gain control over a certain area, thereby creating new boundaries. In both cases the inhabitants of the area will have more contact between one another than with other speakers of the same ancestor tongue situated outside the boundaries. This will create the necessary conditions for convergence between the dialects within the confines of the unit and, as a corollary, lead to their differentiation from the rest. The speakers of the dialects within the boundaries will share innovations, which may cumulate to the point where understanding between speakers of these dialects and their brethren outside the boundaries is no longer possible. There is clearly a stage in the cumulation of changes that must be passed in order that the status of new language be attained. Temporary confederations may also leave traces in the form of isoglosses, but when they are dissolved the tribes will undergo reorganisation, establish new boundaries, and start developing new linguistic units. At a certain point, however, gradual differentiation becomes irreversible; the features distinguishing the given language from its nearest relative will be deep-rooted enough to make it distinct even if later alliance brings the two related languages together again thereby allowing mutual borrowings or even developments in common (cf. the case of Northern Gurage, 3.5.).

Linguistic changes arise as a result of the natural tendency of languages to alter. A change taking place over an entire area of any one of the above-mentioned types may either appear at a certain geographical point, a 'centre of radiation' (Rabin), which has enough prestige to impose its innovations on the rest of the territory or to enforce its archaisms by repressing competitive innovations initiated by other dialects, or it may result from a compromise between conflicting features of the dialects in contact. A new language is created when a certain number of fairly important changes have occurred over practically the same territory. Even if some changes cross the boundary, they do not all do so or they do not do so in exactly the same manner (because if they did they would establish a new boundary). Clearly, the pivotal point of the argument is: which changes are 'important' enough to make the separation of the affected group of speakers from their closest relatives irreversible, to make the boundaries between these and their brethren unambiguously discernible, to convert graduality into discreteness?

When various spontaneous innovations appear on the territory of a dialect cluster some of them are incorporated in later developments and expand further over the entire area, some of them remain within their original confines and, not being too disturbing, do not interfere with further tendencies for split (so that later their domain may even be split in the middle), and some features are suppressed completely due to the prestige of other dialects. Language classification relies on features which have contributed to actual splits and can disregard the other features which, from a synchronic or descriptive point of view, may have the same weight as the genetically relevant ones. Language classification is thus always discriminatory in its treatment of linguistic phenomena, because it tries to select only those phenomena the spread of which foretells the later splits which are otherwise attested. This kind of selection

is always very delicate, since it is circular: those features have to be selected which justify a given division, but the division is itself based on these same features. Therefore, we must assume that there exists a hierarchy of phenomena—some types being more likely to lead to splits and some being more likely to be either dropped or disregarded in this respect.

### 1.2. *Area features*

Since conflicting isoglosses may in one case link language A with B and in another with language C, one may wonder whether there is any justification at all for language classification. We find that certain isoglosses connect languages that are not even related to one another genetically. For example, the labialized realization of long *a* affects the Indo-European languages Tadjik and Persian and the Altaic language Uzbek (for a similar phenomenon in Semitic, see Rabin 1963, 109). Similarly, the word-order of Ethiopian Semitic is practically the same as that of Cushitic (Leslau 1945, 73-8), and the sharp morphological distinction between main and subordinate verbal forms unite Ethiopian Semitic and Cushitic against the rest of Semitic. Vocabulary items are also subject to borrowing between languages of different origin but belonging to the same cultural sphere (e.g. the Norman French elements in English) or continue common substrata (e.g. French *changer* and Italian *cambiare*, both Celtic loanwords). It is specifically these phenomena which either spread in waves or perpetuate features of a substratum on which languages of either different or identical origin are superposed (including the case where the substratum still partially survives alongside the imported new tongues, like Cushitic). These isoglosses may very well be used in typological classifications which disregard genetic origin, but they can only confuse genetic reconstruction. If such isoglosses are found in unrelated languages, or in related languages each of which seems to have otherwise closer affinities with some other languages outside the domain of these isoglosses (e.g. the Balkanic features found in Romance Rumanian, in Slavic Bulgarian, in Hellenic Greek and in Albanian—all Indo-European—as well as in Altaic Turkish), it is easy to see that these features do not contribute to genetic classification. If, however, similar isoglosses are found to cover closely related languages and these isoglosses conflict with each other, one has to establish criteria for deciding which isoglosses are strong enough to define a genetic branch and which ones are of such a nature that they would have affected even languages of different origin so that, in so far as THEY are concerned, the close relationship between the languages involved is merely coincidental.

### 1.3. *Genetic classification*

It would thus appear that there is justification for genetic classification, representable by a family-tree and normally constituting an attempt to reconstruct the development

of the attested related languages from a hypothetical proto-language through splits and branchings due to geographical, cultural and political factors, with or without maintaining territorial continuity. A genealogical tree, with its hierarchical structure, is the graphic representation of a set of hypotheses about the various stages of this development. It is primarily chronological in nature, showing in what order the separate entities which can be identified with later distinct languages appeared on the scene. When building up genealogical trees, one has to make a sharp distinction between isoglosses of genetic relevance and other isoglosses which reflect subsequent changes imposed on already established units or isoglosses which represent spontaneous changes emerging at a given point in the territory of the language, but which do not contribute to later splits into new languages.

#### 1.4. *The importance of morphology*

Let us take the case of Semitic where, with the exception of Ethiopian (Semitic), territorial continuity has been maintained. We have here a large number of contradictory isoglosses, each suggesting a different division. One way of eliminating this problem is to give up any attempt at classification other than a purely geographical one. This has been the most wide-spread attitude among Semitists. However, since the existence of several Semitic languages *is* admitted, there must be some way of reconstructing their development, how they became independent units as opposed to other units. Even if a given language turns out to be the mixture of two different idioms, a *Mischsprache*, practice shows that the contributions of the respective source languages were not exactly on the same scale, one being imposed upon the other (like Norman French on Anglo-Saxon or Central Western Gurage on the Peripheral Gyeto, 3.7.4.), or else the two idioms were more closely related to each other than to other languages so that this is essentially a case of levelling off dialectal differences (cf. Rabin 1963, 115). Now, if we accept the idea so strongly advanced by Rabin that practically all the differentiating changes in Semitic started out from 'centres of radiation' and spread through trade-movements (and not migration), we have to establish 'solidarity-groups' which were the nuclei of later languages (within which dialect mixture was permissible, thus resulting in the development of a standard language). For instance, the adoption of a form *yaqtulu* as imperfect (in what later became Arabic, Canaanite and Aramaic) instead of an older *yiqattul(u)* was obviously a much more important event than, say, the merging of *š* with another sibilant, or the dropping of agreement in number between verb and following subject. Pronunciation and syntactic differences within limits clearly did not disturb mutual comprehension and did not necessarily lead to misunderstanding to the same extent as divergence in morphology. In phonetics and syntax good guesswork could bridge the gap, but this must have been less so in the case of morphemic exponents for which a material knowledge of the meaning was desirable. Now, if permanent contact between an innovating *yaqtulu*-tribe and a still archaic *yiqattul(u)*-tribe necessitated

good communication, one of them had to adopt the imperfect used by the other one to make intercourse smooth. If the second tribe came to adopt *yaqtulu* as imperfect (there being also structural reasons why the innovation should be preferred to the older form), it thereby assimilated itself to the group represented by the first one. And this type of adoption was not simply a case of regular borrowing from one language to the other, because the shape of the imperfect was possibly the only major difference between the dialects and by adopting *yaqtulu* the second tribe did what it could to join the new speech community in process of formation. This, of course, could take place between idioms which, in spite of some differences, were still very similar to one another in most respects. The common adoption of *yaqtulu* thus establishes the group 'Central Semitic' from which Arabic, Canaanite and Aramaic are descended. This division assumes the existence of a period when the differences, from the point of view of morphemic exponents, between the member dialects of this group (not yet the above three branches) were slighter than those which existed between any one of them and the other Semitic idioms. In other words, we could already speak of proto-Akkadian, of proto-South-Arabian-Ethiopian, which did not adopt *yaqtulu*, but not yet of Arabic, Canaanite or Aramaic, which were created by later diversification.

As we have seen, phonetic, syntactic and lexical changes can operate across languages of different origin. In the case of closely related idioms, it is difficult to distinguish changes that have occurred to already established subdivisions from changes that have contributed to the establishment of these subdivisions. We are only interested in changes of the second type, because they reflect the historical formation of languages and are not merely typological items. It would seem that the safest ground for recognizing changes of the genetically relevant type is in the domain of morphology. While common phonetic, syntactic and lexical features may either be more or less late borrowings or the result of a wave-innovation, the probability that a morph found in two or more related languages be borrowed from one of them is very low. This opinion has already been expressed by Cohen (1931, 45), Rabin (1951, 51), and Polotsky (1964, 122). Consequently, such an isogloss is almost certainly indicative of these languages belonging to the same genetic branch—the morph must have been introduced already during the common stage of the branch.

### 1.5. *Morphological borrowings*

Nevertheless, let us examine the possibility of morphological borrowing in order to test out our hypothesis. There are a considerable number of cases where the influence of one language on another must be listed under the heading of morphology. A first reservation to be made is that we have to exclude the cases where the value of a given morph is modified under the influence of another tongue—the linguistic calque. An example of such a calque is given in 1.5.2. Such 'borrowings', affecting the de-

finition of a morpheme and not its phonetic shape, are quite common. Let us try to see whether the phonetic exponents of morphemes can be borrowed or not.

### 1.5.1. *Analogical formations*

There is a form of borrowing which consists in the making use of indigenous material in a foreign manner. One type of nominal plural formation attested in Amharic, Tigre and Tigrinya, consisting of the repetition of the last radical, “was probably favoured in the Ethiopic languages by the influence of Cushitic” according to Leslau (1945, 66). He does not state explicitly the nature of this influence. Moreno (1948, 124), on the other hand, mentions the possibility that this plural formation may be a spontaneous development of the Semitic broken plural, somewhat modified by analogical readjustments.<sup>3</sup> However, both Leslau (1945, 69) and Moreno (1948, 124) agree in stating that the reduplicative adjectives of intensive or attenuative value (e.g. *mālkakkam* from *mālkam* ‘beautiful’) do owe their shape to the impact of Cushitic. In East Gurage and Soddo, the plural formed by means of repetition of the last radical after an *a* is certainly of Cushitic inspiration: Səlti *māre* ‘friend’ pl. *mārarčä*, Soddo *ğärä* ‘hen’ pl. *ğärarä*.<sup>4</sup> However, this type of borrowing was correctly evaluated by Moreno himself when he dealt with the reduplicative verbal stems (1948, 127, also Leslau 1945, 71). The pattern *qätattälä* is not found elsewhere in Semitic, whereas similar derivational patterns ARE found in Cushitic. But what was borrowed was not the phonetic substance of the morphemes, but the method of creating new forms out of existing roots. As Moreno very concisely puts it: “Semitici i materiali, ma cuscita l’artefice”.

### 1.5.2. *Calques*

The distributional features, or uses, of indigenous morph(eme)s may imitate those of other languages. An example is the use of the originally indicative endings of Semitic as Cushitic-inspired main verb markers (3.2.2.). The double negative forms (verbal negation marked by both a prefix and a suffix: Amharic *al-...-mm*, Tigrinya *ay-...-n*, only as main verbs) possibly also follow Cushitic patterns (Leslau 1945, 69-70). Plazikowsky-Brauner (1957, 26) went as far as to suggest that the final *-mm* of Amharic was materially borrowed from Agaw. I would rather suggest that this *-mm* (secondarily geminated) is identical in origin with the connective *-m(a)* which is

<sup>3</sup> For this matter, in the light of recent research, the proto-Semitic character of the broken plurals can no longer be doubted. It is not an innovation in an alleged South Semitic group comprising Arabic and South Arabian-Ethiopian.

<sup>4</sup> For East Gurage, an alternative possibility is that these are borrowings of lexical items along with their plurals, like English *cherub(-im)*. In Soddo, words of Semitic origin may also have this plural: *bəčəl* ‘mule’—*bəčəlalä*.

found all over South Ethiopic and which is most probably Semitic, related to the Akkadian particle *-ma*. The use of such a connective element at the end of a main negative verb is, however, due in my opinion to Agaw influence. In the Southern Agaw dialect investigated by me, there is a particle *-ki* attached to nouns as an emphatic connective element (if attached to two or more nouns successively, it means 'both ... and'), just like Amharic *-mm*. It may also appear (in Agaw, optionally) at the end of negative verbs: *desála(ki)* 'he did not study', also after imperatives *dissé(ki)* 'don't study'. Thus, the distribution of the morph *-mm*, itself of Semitic origin, is a calque on Agaw, with the difference that in Semitic it is restricted to the negated indicative in the verbal system. The same is true for Tigrinya *-n*. Such calques always manifest themselves in distributional features, but the element used in a Cushitic manner is, here again, of Semitic ancestry.

### 1.5.3. *Derivational morphology*

Derivational formatives may be transferred from one language to another, although with somewhat less ease than other lexical elements. Amharic, for instance, has borrowed its abstract formative *-nna*, attached to adjectives, from Agaw (Moreno 1948, 130 and Leslau 1945, 66-67).<sup>5</sup> In the verbal system, the use of the factitive derivative *as-*, in concurrence with the causative *a-*, is quite clearly a calque on Agaw (Leslau 1945, 71 and Moreno 1948, 128). I have further suggested (Hetzron 1971, F.2.) that the element *s* in *as-* has also been materially borrowed from Cushitic. It would seem that derivational formatives may be fairly easily borrowed, being part of lexicon rather than morphology. For the borrowing of derivation procedures, see 1.5.1.

### 1.5.4. *Inferences*

The above mentioned types of borrowing, which might be called 'morphological', can all be shown to be special in a number of quite important respects. In none of them is a phonetic string together with a specific morphological function transferred from one language to another. It is not impossible that real loan-morphemes do exist. An example would be the still contested use of *k*-containing first and second person perfect endings in the Arabic spoken in earlier South Arabian territory (Rabin 1951, 51). As Rabin has pointed out, the first person singular *-k(u)* may be an archaism. On the other hand, the use of *k* in the second persons is clearly an innovation. If these uses are indeed alive in certain areas of the southern part of the Arabian Peninsula, these suffixes were probably borrowed from South Arabian. Nonetheless, the conditions for such borrowings seem to be stricter than elsewhere. They cannot take

<sup>5</sup> Leslau (1945) lists seven such formatives. However, *-čča* and part of *-a* and *-o* do not belong here. These formatives are not productive in these languages. They just happen to occur in several loan-words.



place through contact between neighbouring languages but only when there is a switch of language in the same population. Furthermore, the new and old languages must be related to each other and the speakers must also be aware of this fact.<sup>6</sup>

There may be other cases of borrowing of this type. Nevertheless, the fact remains that in using morphological innovations (as far as their phonetic shape is concerned) as decisive isoglosses, we run less risk of failing to recognize borrowings than in any other domain of the language.

#### 1.6. *Morphemic innovations* (cf. Greenberg 1957, 49 sq. on 'innovations')

If in genetic classification one is willing to lay special emphasis on morphology (which does not, of course, mean that everything else is excluded from consideration), one must still be wary of archaisms which are liable to survive in unconnected areas (the centre being innovative). For example, an imperfect having a vowel *a* between the first two radicals is attested in both Akkadian (*ikaššad*) and Ethiopian Semitic (*yäsäb(b)är*). This does not, however, necessarily mean that they belong to the same branch of Semitic. The form in question was present in proto-Semitic and is a survival in these two peripheral areas (Cantineau 1932). On the other hand, the corollary that the innovative form *yaqtulu* is found in Arabic, Canaanite and Aramaic is an indication of their belonging to the same branch. Now, the *yäsäbbär* form of Ethiopian and the *yaqtulu* form of Arabic may have had the same use and meaning, but the isoglosses in question do not refer to the category,<sup>7</sup> but to its actual exponent.

Consequently, the safest type of genetically relevant isogloss is a morphological innovation, a new phonetic exponent of a morpheme. A morphological category may arise under the influence of other languages, such as a special 'compound' form for the main imperfect in South Ethiopic (3.2.). This, in itself, does not prove anything. The Cushitic influence may have been exerted on each language separately. However, we can use as an important isogloss in the establishment of the two branches of South Ethiopic the fact that, for creating such 'compound' forms, one group of languages used the Semitic indicative morpheme *-u/-n* by reinterpreting its function (Hetzron 1968), while the other group used the auxiliary *hlw* the original function of which was, according to the testimony of modern North Ethiopic, that of forming continuous tenses. When such a morphological innovation concerning the exponent of the morpheme (that is the morph) is found in a number of languages, one can posit that the innovation took place in their common ancestor, hence we can infer that they did have a common ancestor. The requirement of identity of exponent and not identity of category guards us against universal tendencies and parallel but independent influences of the same substratum. The requirement of innovation guards

<sup>6</sup> I remember a story about an Israeli child staying in Ethiopia who, realizing the similarity of the verbal prefixes of Hebrew and Amharic, constructed his own hybrid form *yistovevall* from Hebrew *yistovev* 'he will turn' and Amharic *-all*, the main verb marker.

<sup>7</sup> For this matter, the form *yäsäb(b)är* is a present-future in Ethiopian Semitic, except in Central and Peripheral Western Gurage where it is only a present.

us against scattered survivals of ancient phenomena which were elsewhere replaced by innovations.

For instance, the fact that the present perfect is derived from the converb (gerund) in Amharic, Argobba, East Gurage, Soddo, Gogot and Mäsqaṇ is indicative of similar substrata only (cf. Moreno 1948, 128-129). On the other hand, the fact that the auxiliary *-all/-an* was selected arbitrarily in the first three (Amharic *säbro* 'he broke/breaks and', *säbroall* 'he has broken', 3.3.1.), while in the second three the present perfect and the past converb are homonymous and only positionally distinguished (*säbbärä-m* 'he broke and' in non-final and 'he has broken' in final positions) may be used along with other features when establishing a genetic classification.

## 2. THE ORIGIN OF ETHIOPIAN SEMITIC

There is no doubt that the independence of the Ethiopian branch of Semitic was established through migration and subsequent mingling with the indigenous population. The closest relative of this branch is Modern South Arabian. The existence of a vowel between the first two radicals in the imperfect is an archaic feature which opposes these two to the innovative Arabic (with *yaqtulu*). The second person suffixes of the perfect in *k* (as against *t* elsewhere) are an innovation which sets these branches apart (Cantineau 1932, 180-181). Unfortunately, the absence of vocalization and the lack of attested second person perfect forms in Epigraphic South Arabian prevent us from drawing safe conclusions concerning the relation between this group and the above two but, in all likelihood, they all belong to the same branch.

In view of the great diversity which exists between the Semitic languages of Ethiopia, it is probable that the Semitic immigration started much earlier than is usually posited (cf. A. F. L. Beeston's remark in Ullendorff 1955, 8, n. 30). The earliest attested Ethiopian Semitic language, Gə'əz, lacks certain Semitic features found in other Ethiopian languages and shows innovations not found in other languages.

Such features are: the Gə'əz third person plural suffix pronouns are m. *-(h)omu* f. *-(h)on*, representing proto-Ethiopian m. *\*-hämu* f. *-hän*, while all the other languages continue the older type with *ä*, so that *o* is only a Gə'əz innovation;<sup>8</sup> the Semitic negation morpheme *al-* does not exist in Gə'əz (nor in the Tigre dialect of Mensa, although it does in Bogos) except in the archaism *albo* (Tigre *aläbu*) 'there isn't', while it is THE negation morpheme in all the other languages;<sup>9</sup> South Ethiopic (Northern Gurage, traces in Peripheral Western Gurage, Gafat and Amharic) has a main verb marker, a descendant of Semitic indicative endings (3.2.2.), of which there is

<sup>8</sup> I think that Tigrinya and Tigre third person masculine plural *-om* comes from *\*-ämu* and not *\*-omu*. But even if this is contestable, it is still obvious that the third person feminine plural forms *än* contain no labial element.

<sup>9</sup> *al-* or its developments, *ay-*, *an-*, *äl-*, *a-* + gemination of the subsequent consonant; see Hetzron (1972), H. 2.



absolutely no trace in Gə'əz. These and other features suggest that by the time Gə'əz was codified (1st century A.D. according to the estimate of Ullendorff 1955, 9) the ancestors of all the other languages, including its closest relatives, were already separated from it.

The other question is whether or not we should posit a common origin for all the Ethiopian Semitic languages. I agree with the general opinion that the answer is in the positive. Although it is logical to assume that the South Arabian immigration itself was not linguistically completely homogeneous (Ullendorff 1955, 14), there is no reason to believe that completely different types of speech were imported onto African soil. Furthermore, the act of settling in Ethiopia and early Cushitic influence both served as efficient homogenizing factors. A number of features common to ALL the Ethiopian Semitic languages but not found elsewhere in Semitic are probably all due to the early influence of Cushitic and argue for monogenesis. One such feature is the existence of composite verbs with  $x + \text{'to say'}$  for a variety of verbal expressions (Gə'əz *ənḃayä ḃəhil*, Amharic *əmḃi alä* 'to refuse'), all using developments of the root *\*bhl* (still rare, but attested in Gə'əz, cf. Leslau 1945, 72). Such composite verbs are widely found in Cushitic, but it is unlikely that the transmission took place separately for each subgroup. Some features are not found in all the languages, but they are found in representatives of each branch while not in the closest relatives of these. Such a feature is the use, employing a Semitic form according to a Cushitic pattern (i.e. calqued on Cushitic), of the converb (gerund) instead of sentence co-ordination. The converbial constructions are common in all the Ethiopian Semitic languages and were already so in Gə'əz, but the original converbial forms (based on the Semitic pattern *sābir(ä)-*) are found today (in addition to Gə'əz) in Tigrinya (but not in Tigre), Amharic, Argobba and, with a limited application, in Central and Western Gurage (with a modified pattern *səbirtä-*, see Polotsky 1951, 45-46 and Hetzron 1972, U.3.) and Gafat. The existence of converbial constructions is only indicative of a common Cushitic substratum, but the existence of the original form (as confirmed by Gə'əz, the least Cushiticized) in all the branches makes it probable that those languages which do not have it (Tigre, Harari, East Gurage) have lost it so that the calque operated already in proto-Ethiopic. Another calque also attested only in these languages is the fact that the verb meaning 'there is' (developments of the root *hlw*) is conjugated in Gə'əz, Tigrinya, Amharic and Western Gurage as a perfect, has the meaning of the present, and admits the temporal prefix (*ənḃä/k-/s-/t-* respectively) which is confined to imperfect forms for other verbs. This is probably a calque on Cushitic, as suggested by the situation in Bilin (Hetzron 1972, F.5.). This suggests that there was a proto-Ethiopic language and that the ancestors of all the Ethiopian Semitic languages came through the same 'sieve'.

How shall we deal, in the light of all this, with the distinction between North and South Ethiopic, the two main branches of Ethiopian Semitic? If we accept the thesis of monogenesis, we must assume that the Semites entered Africa through the area now occupied by the North Ethiopians. North Ethiopic is then, territorially, a

continuous survival of proto-Ethiopic. In early times, proto-Ethiopians spread toward the South and, by freeing themselves from the influence of the north and by mixing with the local Cushitic population (in which they were numerically a minority), they became the ancestors of the South Ethiopians. Features shared by all the South Ethiopic languages (3.1.) suggest that they originally constituted a territorial unity, southwest of the proto-Ethiopic area. Later, these southerners sent a wave of people further southward. Separated from the original stock, this vanguard became the Outer South Ethiopians, represented today by the Northern Gurages (a continuous survival of proto-Outer South Ethiopic), the Gafat and the Western Gurages. The remaining southerners started to spread more slowly along a south-eastern route: the Transversal South Ethiopians with Harari and East Gurage at the extremity, Argobba in the middle, and Amharic still in the north. The Amharas migrated least and remained in contact with the northern Aksumite civilization, after the destruction of which they took over political hegemony in Ethiopia.

The territorial continuity between Harari and East Gurage was later disrupted by wars and population movements. There is also evidence that Gafat and Soddo had once been neighbours. Later, the Galla invasion reduced the Soddo domain and Gafat yielded to Amharic.

The following genealogical tree (TABLE 2) shows the genetic relationship as reconstructed between the Ethiopian Semitic tongues. Section 3 contains a summary of the justifications for the branchings presented. More details are given in Hetzron (1971). The names of languages are italicized. Gə'əz is extinct, probably Gafat also.

### 3. THE DIVISIONS OF ETHIOPIAN SEMITIC

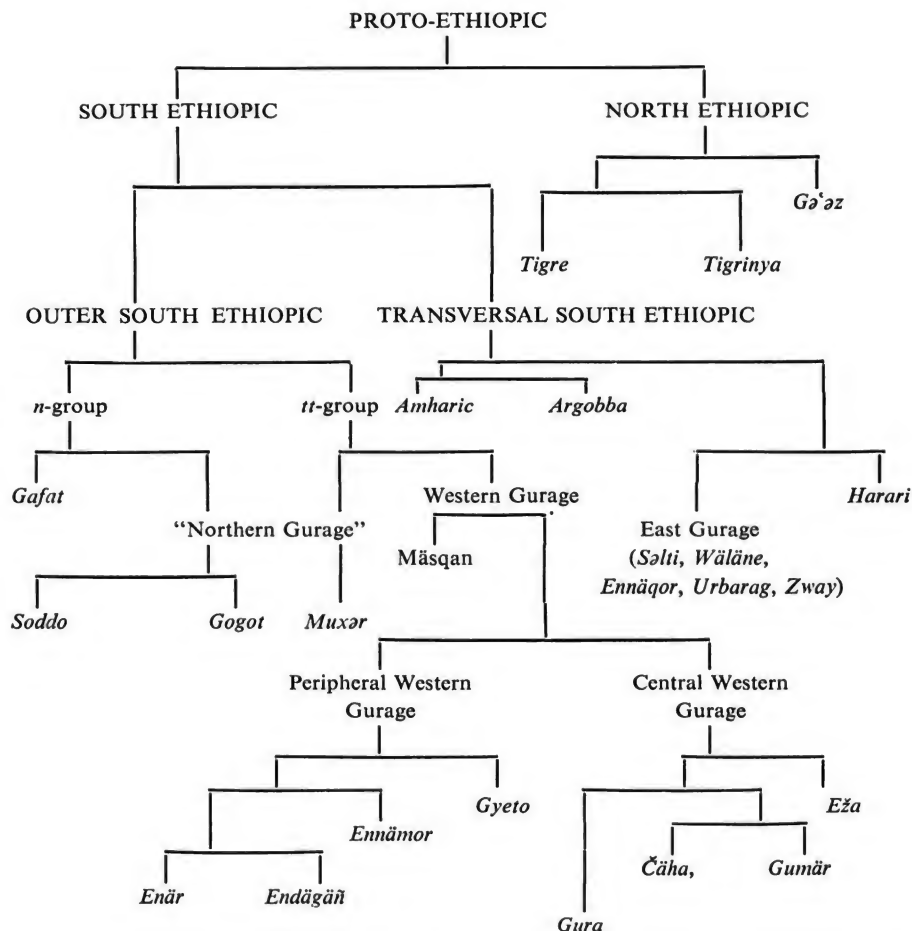
#### 3.1. *North versus South*

The best isogloss to distinguish between North Ethiopic (NE) and South Ethiopic (SE) is found in the verbal system, namely the presence or absence of gemination in the second radical in the simple basic stem (type A) and the distribution of the palatal vowel *e* in the geminated basic stem (type B). The following forms are the third person singular masculine prototypes from which all the extant forms may be derived (cf. Polotsky 1949, 38). The Northern forms are actually attested in this shape in Gə'əz and the Southern ones, with one small modification, are the forms used in Argobba. The roots are *sbr* 'to break' and *fšm/fʔm* 'to accomplish'.

TABLE 1

	NE		SE	
	Perfect	Imperfect	Perfect	Imperfect
Type A:	<i>säbärä</i>	<i>yäsäbbär</i>	<i>säbbärä</i>	<i>yäsäbär</i>
Type B:	<i>fäššämä</i>	<i>yafeššäm</i>	<i>fettämä</i>	<i>yafettäm</i>

TABLE 2



In type A, NE has a non-geminated perfect and a geminated imperfect; conversely, SE has a geminated perfect and a non-geminated imperfect. In type B, NE has the palatal vowel *e* in the imperfect only, while palatality became the general exponent of type B in SE (Polotsky 1938, 151 and 3.3.2. below). This palatality was subject to further changes, even to complete reduction in some cases. Gemination was reduced altogether in most Western Gurage languages and in the verbal system of Harari and, with exceptions, in East Gurage.

It is obvious that the NE situation is the archaic one and that the SE system is only an innovation (Polotsky 1949, 38). For an attempt at a historical explanation, see Hetzron (1972), H.1.

Another feature which distinguishes NE from SE is that in the latter, under Cushitic influence, a sharp morphological distinction is made between main and sub-

ordinate verbal forms. This development, however, took place later, and it triggered the split of SE into two branches.

### 3.2. *The subdivision of SE*

The main basis for this subdivision is the element adopted to mark main imperfects, as opposed to subordinate ones.

3.2.1. The languages that use developments of the root *hlw* 'there is' (Amharic, Argobba, Harari *yäsäbr-al(l)*, East Gurage *yäsäbr-an*) are the Transversal South Ethiopic (TSE) languages (cf. Cohen 1931, 192-193). They are: Amharic and Argobba on the one hand, and Harari and East Gurage on the other (cf. 3.3.2.). These languages have also lost all mark of gender in the plural of verbal forms (Leslau 1951, 217).

3.2.2. In the other branch, the Outer South Ethiopic (OSE), the original Semitic indicative markers *-u* (after consonants) *-n* (after long vowels), probably augmented by a final *-t* to give *-wt/-nt*, were reinterpreted as main verb markers and also extended to the perfect. These markers have survived in Northern Gurage (*yäsäbr-u*), but have disappeared elsewhere leaving, however, clear traces in Gafat and Peripheral Western Gurage (Hetzron 1968). These languages have also reshaped their verbal plural markers from Semitic (Gə'əz) m. *-u*, f. *-a* to *-mu/-ma* (Polotsky 1938, 163-164). They have also maintained the feminine plural (see however 3.6.).

### 3.3. *Transversal South Ethiopic (TSE)*

This branch can be further subdivided into the Amharic-Argobba cluster and the Harari-East Gurage group.

3.3.1. The extremely close relationship which exists between Amharic and Argobba is quite obvious (Leslau 1960, 93-98). The present perfect is formed in TSE by means of the converb and an auxiliary coming from the root *hlw*. Now, quite arbitrarily, the auxiliary is conjugated in these two languages only in the first person singular common and in the third person singular feminine (optionally in the first person plural common in Argobba), but is invariably *-all* elsewhere. This 'coincidence' can only be attributed to common development. The relative particle before imperfect is *yämm-/əmm-* with a mysterious *-mm-* which occurs only in these two languages.

3.3.2. Harari and East Gurage use the imperfect combined with a development of *hlw* (3.2.1.) also as a relative (Wälāne *yäsäbr-an* 'he breaks' or 'who breaks', but with some difference in Harari (*yäsäbrāl* vs. *yäsäbrizāl*) and in Zway, and as a quotative (East Gurage only, Wälāne *yäsäbr-an-ko* 'that he breaks'). These languages

have dropped gemination in the verbal (but not the nominal) system. They have also extended palatality as an exponent of type B (3.1.) to the jussive and the infinitive (Wälāne 'to follow': perfect: *täketälä*, imperfect: *yäketäl-an*, jussive: *yätkitäl*, infinitive: *täkitälot*).

### 3.4. *Outer South Ethiopic (OSE)*

These languages used the main verb markers of Semitic origin (3.2.2.): *\*-wt/-nt*. The latter allomorph *\*-nt* either became *-tt* through assimilation or *-n* by dropping the final *t*. This constitutes the basis for a further subdivision into a *tt*-dialect group and an *n*-dialect group, already present in proto-OSE.

The second person and first person plural common complement suffixes split into two allomorphic sets in OSE, maintaining the original form after consonants and short vowels ('light' suffixes) and with secondary gemination of the initial consonant of the suffix after original long vowels (in the process of shortening) ('heavy' suffixes), thus *\*yäsäbər-kä* 'he breaks you (m.sg.)' and *yäsäbrämu(:)-kkä* 'they break you' (Polotsky 1938, 160-162 and 1951, 29 ff.). Because of incomplete data, the Gafat situation is descriptively not clear, but the existence of these two sets can be proved beyond doubt (3.6.). Allomorphs of the first person singular common and third person object suffixes continue the proto-Semitic allomorphic distribution (e.g. *yäsäbr-e* 'he breaks me' (light) and *yäsäbrämu-ṇṇ* 'they break me' (heavy), cf. Hetzron 1969).

A morphophonemic phenomenon attested in all the *tt*-group, although not with exactly the same distribution, is the internal labialization (Polotsky 1951, 39). Because of the reduction of suffixes originally containing or consisting of a long labial vowel, certain morphemes should be described as consisting (at least in part) of a labial appendix super-imposed upon the relatively last labializable (labial or velar) consonant, if any (e.g. *yäsäbər* 'he breaks' + 'him' = *yäsäb<sup>w</sup>ərr*; for details, see Hetzron 1971).

### 3.5. *Northern Gurage*

Northern Gurage is a continuous survival of proto-OSE, and still contains the original dialectal division into *n*-group (Soddo and Gogot) and *tt*-group (Muxər). Although Northern Gurage is not a 'branch' genetically speaking, the constituent languages by staying together developed innovations in common distinguishing them from the two side-branches. This is an example of convergence after irreversible split between related languages. The three Northern Gurage tongues adopted the Semitic root *ḥlf*, originally meaning 'to pass', for 'to go' (*alläfä/annäfä/efä*), whereas Western Gurage and Gafat maintained the original root *ḥwr* (*wä(ä)rä/horä*). They also dropped the element *t* in the third person masculine and feminine independent pronouns (*k<sup>w</sup>a/k<sup>y</sup>a* as against Western Gurage *huta/hita* and Gafat *wət/yət*; cf. Gə'əz *wə'ətu/yə'əti*).

3.6. *Gafat*

This very recently extinct language (Leslau 1956) had no feminine plural, but its only plural ending shows the OSE innovation: *-im<sup>w</sup>* (3.2.2.). A striking incongruity held in common with the other OSE languages is that after the first person singular common perfect, object suffixes of second person are of the heavy (geminated) type, but the third person objects are marked by *n*-containing (light) suffixes (3.4., Leslau 1956, 62, cf. Polotsky, 1938, 161). A trace of the main verb markers and some other features identify it as belonging to the *n*-group (Hetzron 1968, 170-171).

3.7. *Western Gurage (WG)*

This group is an outgrowth of the *tt*-dialects, judging from the survival of the main verb markers after the past tense of the verb 'to be' in Peripheral Western Gurage (Hetzron 1968, 169-170) and other isoglosses. It is subdivided into Mäsqaṇ and another group which is in turn subdivided into Central and Peripheral Western Gurage (CWG and PWG).

3.7.1. *Mäsqaṇ*

The third person singular masculine and feminine independent pronouns are *huti* and *hiti*, with final *-i* as against a final *-a* elsewhere in WG. Mäsqaṇ is the only WG language to have the following tense system:<sup>10</sup> past (perfect) *säbbärä* 'he broke', present perfect *säbbärä-m* 'he has broken' and imperfect *yäsäbär* 'he will break' (cf. 3.7.2.). The verbal plural marker is *-o* as in CWG and Gyeto.<sup>11</sup>

3.7.2. *The CWG-PWG group*

The other branch of WG has, like Muxər, adopted the original present perfect form *säbbärä-m* as a past (and no longer has a present perfect). As Muxər also participated in this development, this change must have occurred after the first branching of WG (3.7.1.), as an area phenomenon. The feature, on the other hand, which underlies the first branching is that the imperfect *yäsäbär* became a present only, and two future forms were evolved: (1) indefinite future, 'jussive + *-šä/-se*', used for future tainted with subjective feelings (hope, expectation, fear, doubt, wish, etc.), and (2) definite future, 'imperfect + *-te/k<sup>w</sup>e*' (3.7.4.), for statements the future character of which is taken for granted.

<sup>10</sup> Except for the main verb markers, this is the same as in Soddo and Gogot.

<sup>11</sup> Probably through the merger of the original subject ending *\*-mu* and the third person masculine plural object suffix *-ämu*. The latter form was then used in both functions and phonetically reduced to *-o*. This seems to be a late innovation after the split into branches.

### 3.7.3. *Central Western Gurage*

The following feature is found in CWG but not in PWG: definite future is always expressed by means of the suffix *-te* after imperfect (a simplification on the part of CWG). The following features are also found in Gyeto, but not in the other PWG languages: light third person object suffixes contain an element *-n-* and are concomitant with internal labialization (3.4. and Hetzron 1971, 5.3.), which is an archaism (Hetzron 1969); the third person plural masculine forms of the verb have a suffix *-o*, an area innovation (n. 11); indefinite future is marked by *-šä*.

### 3.7.4. *Peripheral Western Gurage*

These languages have the most complicated verbal morphology in the whole of Ethiopian Semitic. The following features are attested exclusively in this group. Nasalized vowel and *ř*, *ṁ* and *ṃ* have emerged. The Semitic laryngeals ' and ' partially survived in the shape of a ' intervocalically or as a second member of a cluster whose first member is a labial or a lateral continuant. In compensation for the loss of the laryngeals *\*h* and *\*x*, and the nasal *m*, phonologically relevant long vowels appeared (Leslau 1959 and Hetzron 1970). The definite future suffix has different allomorphs conditioned by the preceding element: *-te/-de* and, according to the dialect, *-k<sup>w</sup>e(y)/-ke*. There are minor differences between dialects in the distribution of the allomorphs. Further suffixes *-ta/-da/-ka*, with the same distribution as above, are used to form the main negative imperfect and perfect, relative imperfect and stative (out of the temporal in *t-*, *tisäβar* 'when he breaks', *tisäβarka* 'while he is breaking'). This is an innovation on the part of PWG.

Gyeto also shares the above features (except for *ř* and *ṃ*, traces of which are however found). Here are some features of PWG not found in Gyeto (nor in CWG). Light third person object suffixes after imperfect contain an element *-k-* and are not accompanied by internal labialization (3.7.3., cf. Hetzron 1971), a late innovation. The third person plural masculine has internal labialization, its suffix is *-ua* in final position, but has no suffix before other suffixes (*yäsawar(ua)* 'they broke' sg. *yäsäβar*, while CWG and Gyeto have the third person masculine plural *yäsäβro*, cf. n. 11). This is an innovation probably simultaneous with the one described in n. 11, after the split into branches. In these languages the third person plural masculine form is thus homonymous with the impersonal.

Since Gyeto shares several innovative features with PWG which are not likely to have been borrowed by contact (especially the above mentioned phonetic subtleties) it must be considered as a PWG language. The isoglosses held in common with CWG and the PWG features not found in Gyeto must be explained by positing that proto-Gyeto came into early contact with CWG, separated itself from the rest of PWG (which was to undergo further changes, innovating the light third person object suffixes after imperfect, the third person plural masculine form), maintained

archaisms dropped by other PWG languages (the light third person object suffixes), and participated in further developments of CWG and Mäsqa (n. 11).<sup>12</sup>

### 3.8. *Closing remarks*

The features and phenomena enumerated above do not give a full picture of these languages. Only those elements that were deemed to be good isoglosses and which provided a clear illustration of the method described at the beginning of this paper have been mentioned. There exist other isoglosses that seem to conflict with our classification and some of these have already been presented here as area features. However, all these contradictory isoglosses belong to the categories which are likely to be superposed on a number of closely related, but already distinct, languages. All these features are negative ones, not the development, the rise, but the loss of some element over a continuous area. Some of these features are: lack of plural marking in nouns in the *tt*-group and Gogot;<sup>13</sup> disappearance of the present perfect as a category (its original form being used for the new perfect) in the *tt*-group except Mäsqa; devoicing of geminates in PWG and CWG except Eža (Polotsky 1938, 140), accompanied by loss of gemination except in Enär and sometimes in Endägäñ (Eža *säbbärä-m*, Čäha *säpärä-m*, Ennämör *säpär'ä*, Enär *säppär'ä* 'he broke'). For a detailed account of all the above mentioned features and many more, see Hetzron (1971).

## 4. THE ETHIOPIAN SEMITIC LANGUAGES

The Ethiopian Semitic languages are: A. North Ethiopic: Gə'əz on the one hand, and Tigre and Tigrinya on the other; B. South Ethiopic: (1) Transversal South Ethiopic: (a) Amharic and Argobba, (b) Harari and the East Gurage dialect cluster (Solti, Wäläne, Ulbarag, Ennäqor, Zway); (2) Outer South Ethiopic: (a) the *n*-group, with Gafat on the one hand and Soddo and Gogot of Northern Gurage on the other, (b) the *tt*-group, with the third Northern Gurage language Muxər, and the Western Gurage branch, subdivided into Mäsqa and another subbranch further divided

<sup>12</sup> It is remarkable that the PWG features not found in Gyeto are both connected with the fate of the internal labialization. For a historical explanation, see Hetzron (1971).

<sup>13</sup> Murtonen (1969) expresses the idea that these languages did not lose their plural declension, they never possessed it. He further claims that "there is *no* known instance of a language having given up plural declension secondarily ...". However, for a few nouns these languages do have suppletive or broken plurals (Hetzron 1971, n. 50), a clear trace of the earlier existence of a plural declension. On the other hand, there is a well-known instance of a language having given up plural declension, a language as little exotic as spoken French which, with the loss of final *s* has lost its plural marker, except for part of the nouns ending in *-al* or *-ail* (plural *-aux*, orthographically). Number is marked, like in Gurage, by determinatives—when possible, but not, for instance, after *beaucoup de* ... The final *-s* is marked in the script but, as is well known, is not pronounced except in some cases of *liaison*,



into the Central Western Gurage dialect cluster (Eža, Čäha-Gumär, Gura) and the Peripheral Western Gurage dialect cluster (Gyeto somewhat apart, Ennämor, and Endägañ with Enär). Northern Gurage is an *a posteriori* unit, a continuous survival of proto-Outer South Ethiopic.

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## DISCUSSION

PALMER: One small point concerning classification, and that is the division into North and South Ethiopic. Frankly I don't think this is a particularly useful or particularly valid distinction. It is rather like saying that we have North and South Romance taking Spanish and Italian as being South and French as being North. In the case of the examples that Hetzron gives the evidence really is very weak indeed. He takes two points only, in section 3.1. of his article. One concerns in effect the syllabification, the question of gemination of the consonants; but what he omits to mention is that, although you do get in the imperfect of the Northern ones a geminated consonant in the example given, once you have a form with a vowel on the end the consonant is no longer geminated. So it is only geminated in part of the forms and one would have guessed that the gemination of these consonants was accounted for by simple syllabic, that is phonological, conditioning. The second point is even rather odder, where he is talking about the occurrence of the *e* vowel in the imperfect form, and on this occasion he quotes Gə'əz forms against Argobba. In fact, if you look at the two modern North Ethiopic languages, Tigre and Tigrinya, you don't get this. So this would imply, if anything, that these two did not belong together with Gə'əz but in fact that Gə'əz belonged much closer together to the Southern Ethiopic. I don't say any of this evidence is totally inaccurate, but at the same time it is very much less powerful than it stands. He has only given us two points anyway of a very minor kind and both of them are only half truths, and I think we ought to be very much more careful. There are other areas—Cushitic, as I shall make my own point tomorrow — in which classification is very often very haphazard, and in this particular case I am not sure that very much is added by labelling the languages of Ethiopia 'North' and 'South'.

LESLAU: I don't have to tell you that this subject is very close to my heart. I am again sorry that Hetzron is not here to discuss the problem of principle; consequently I do not want to enter into details. There is, first, the question of interpretation. Now interpretation is subjective. As long as the facts are precise and true, any one is entitled to his own interpretation. In connection with the question that Palmer brought up concerning gemination or non-gemination of the second radical of the imperfect—North Ethiopic with gemination, South Ethiopic with no gemination—of what is called type A or Qal in Hebrew. Now which is the proto-Ethiopic type? It is a question of interpretation. I happen to disagree with Hetzron on this point, I think South Ethiopic continues the proto-Ethiopic form (that is, no gemination of the second radical) and North Ethiopic has innovated. But this is a question of interpretation, as I said. Something that is in between interpretation and fact is the question of what became known as the pseudo-gerundive, a term first introduced by Polotsky and then taken up by me personally. It is a kind of form—I don't want

to enter into details—but the base of it is, oddly enough, the second person feminine of the imperative. I agree it is odd and I do not know its origin. Now Hetzron wants to see a base *səbir* in order to put it together with the other type of gerundive, *säbir*. But what is characteristic of this pseudo-gerundive is that any palatalizable last consonant—dental, sibilant, velar—can be palatalized. Now this happens in Ethiopic only if the vowel *i* follows and not precedes; consequently a *säbir* cannot bring about a palatalization of the last radical. So this is a question which lies between fact and interpretation. Now, concerning facts, why just take occasionally, as has been mentioned already, one point and make it a basic criterion for classification? For instance he takes the imperfect, subordinate or main, and bases his classification on it; in the case of Gurage, Muher is for Hetzron North Gurage because it behaves like Soddó. But Muher has ten other features which it shares with West Gurage, and two very important ones such as two jussive types—very important because it is found only in Gəʾəz and only in West Gurage. Muher has it too. Now why should the form of the imperfect, main or subordinate, be given more importance as against the ten other features in classifying Muher either with West Gurage or with North Gurage? Or again, a question of fact, in what Hetzron calls Central West Gurage he treats as basic the following feature, namely the existence of a definite future, imperfect + *te*. Now it so happens that, within what is according to him Central West Gurage, Ennemor does not have this kind of future. Or again, within the same group, Gurage, speaking of very closely related languages such as Ennemor and Endegeñ, he states something to the effect that in verbs with a laryngal as second radical—let's say for the root 'to eat'—the laryngal is kept as a glottal stop. Now Endegeñ, which is the closest related to Ennemor, does not have this glottal stop but has a biradical root, and so on. What I mean to say is again concerning the general problem of classification. Roughly speaking we know how the situation is. It was first established by Marcel Cohen. Whatever one may think, I still agree with a wise distribution of North and South because there are certain features that we have in the South, but to go into this seemingly impressive classification within Gurage such as Peripheral Gurage, Central Western Gurage, Transversal South Ethiopic, and so on may sound impressive and interesting, but the danger is that someone from outside—and I mean here our colleagues anthropologists and others—will jump on the occasion and make their own classification on the basis of what is done in linguistics. It is dangerous and it should not be done unless the whole documentation is in front of us. And the documentation is not there, especially in the complicated group of Gurage for which we have twelve dialects and the interplay between these dialects is so enormous—you find two closely related dialects and yet one feature is there and another feature is not there. Tomorrow, when we speak about Cushitic, we will see that the Survey of East African Languages has taken up this division and, rightly or not, an outsider will see this East Africa Survey and will go to it to find what is the latest on the division. And this will be

perpetuated because of the sacred nature of every written word. So one has to be extremely careful with classification unless all the data are with us.

PETRAČEK: Our colleague Hetzron has dealt with certain methodological problems but if I am not mistaken these problems should be resolved prior to their application to the Semito-Hamitic field. When discussing the wave theory Hetzron says for instance that languages are not necessarily insulated entities. Now this is true, if you like, from a genetic point of view but from a structural point of view it is not. They are of course not absolutely closed systems, but still systems. One has to resolve the relation between history and system. Then the question of hierarchy of phenomena: we are well aware of hierarchical structure in language but it is an entirely different and very difficult problem indeed to say what the hierarchy is with respect to any particular language. The question of area features next: apart from the name the concept is familiar to us under the name of *Sprachbund* as developed by Jakobson and Trubetzkoy. Now, although useful, this is an extremely vague concept and one has to first of all define what an area is. Also, it seems to me that our colleague Hetzron overestimates the morphology. In a linguistic system every element is important; of course one has to know its position, in the centre or in the periphery, but each phonetic, lexical, etc. element matters. And concerning finally the application of morphological borrowings, analogical formations and other concepts one could discuss many things. The repetition of the last radical for instance as a means to form plurals we find, according to Greenberg, also in other languages such as Berber, which suggests that it is perhaps not analogy but rather an inherited trait. Or the question of the negative formed by means of two morphemes before the second but last radical: we know the phenomenon from Arabic *mā-*, *-š*, etc. All these questions require discussion but since unfortunately our colleague is not present I am not pursuing the matter now.

BENDER: Since I am largely responsible for the fact that the Language Survey of Ethiopia has chosen to publish Hetzron's classification as part of our monograph I would like to explain why this is so and answer two other objections of Professor Leslau's. First of all, Hetzron tries to make it clear in this paper that he is choosing INNOVATIONS in a very particular way. In other words I don't think anybody here seriously objects to the idea of using shared innovations in making genetic classifications of languages. Hetzron points out that innovations can be more or less valuable for classification and he tries to choose innovations which are more highly valuable in the sense that they are better for purposes of classification and he explains what he means by this in the first part of the paper. Secondly as for our publishing this scheme. It is a new scheme which seems to me and to some of us to make a lot of sense and we would like to present it for people's consideration and we are certainly not endorsing it and saying that this is the final answer. Finally also I think it was overlooked that in here Hetzron several times mentions that in his fuller paper

which is being published as one of the Journal of Semitic Studies' monographs he gives much more documentation which he can't possibly give in this brief paper that he prepared for presentation here.

FENTON: I should like to refer in particular to Hetzron's paragraph 1.4. In agreement with Dr. Bender and with great respect perhaps in disagreement with Dr. Petráček, I would agree that certain features are of greater importance, especially certain morphological features. And for that reason it is important to get the facts concerning morphological features right. What I am going to say I think will touch a number of points which have already been made in several ways. I regret both that Dr. Hetzron is not here and that I must refer to as yet unpublished material of my own, but it will be out soon, in the next number of the Journal of Semitic Studies. We have here the assumption, taken as proven already, that the form *iqattal* is proto-Semitic and that the Canaanite languages and Aramaic rejected the form *iqattal* and created the new *yaqtulu*. Many articles have been written on this. I must say however—*pace* Professor Garbini—that I find the evidence for this non-existent. On the contrary one can prove that Ugaritic for example had no *iqattal* form and one can show that other cases which have been alleged—in Hebrew by Rössler, and in other dialects, more recently neo-Punic—these examples do not in fact occur. On the contrary features of the language exist which preclude their existence. I would therefore say that perhaps Ethiopic scholars should look again at the fact that the equivalent of *iqattal* does not seem to be semantically equivalent to the Akkadian usage and possibly we have to deal with a different development. However, I would perhaps mention that if the *iqattal* was a proto-Semitic feature which died out before the formation of Western Semitic, we have to deal with a situation where the most peripheral languages have a feature which was created only in certain areas of Semitic however tenaciously it was retained in those areas once it had been created. I find it more likely myself that the feature is not proto-Semitic—it never existed in Canaanite—and that it was diffused only in certain areas if indeed it is not, as I have mentioned, a quite different feature and that the Ethiopic and perhaps the Berber area have a usage which is quite different from the Akkadian usage. I think this colours the whole presentation of the relationship of languages which Hetzron suggests here.

HETZRON (by letter): In reply to Professor Palmer's criticism, I am quite aware of lack of gemination before suffix in the Tigrinya and Tigre type A imperfect. Yet it seems to me that both historically and descriptively the correct statement is that this form HAS gemination in the second radical, but loses it before a suffix,—rather than saying that this form has no gemination, but obtains one when there is no suffix. Historically, it must have LOST gemination before a suffix, since there is no valid justification for DEVELOPING gemination in a pre-no-suffix position. It is simply not true that 'the gemination of these consonants was accounted for by simple

syllabic, that is phonological, conditioning'. This statement would imply that all pre-VC# consonants should be geminated in Tigrinya, which is not the case. Gemination is not conditioned,—its lack is. And this makes a great deal of difference. Furthermore, I am not talking about 'the occurrence of the *e* vowel in the imperfect form' of type B, but about the fact that both tenses have the same vowel between the first two radicals in the South, but not in the North. In fact, Tigre has the same *ä*, but the innovation I am basing my division on is the generalization of *e* for all the tenses in the South. Likewise, for gemination, my point is that the South adopted gemination in the PAST of type A, which is definitely an innovation. Thus, by no means does Gə'əz belong together with the South, for it has *fäṣṣämä* in the past. Reply to Professor Leslau: Whether the geminated imperfect of type A is the original one or not may very well be a matter of interpretation, both on my part (who am claiming that gemination is original) and the other one (which considers gemination to be a Northern innovation). Yet there is evidence in support of the originality of gemination. First, there is the relationship with Akkadian *ikaššad* with the gemination. Then, in the passive even Amharic has gemination in the imperfect type A,—a fact that has been, but should not be, disregarded. Furthermore, in IIw verbs, the more classical type of Amharic has past *honä*, nonpast *yəhon-*, converb [= gerund] *huno* (though the modern trend is to use *hono*). Now, if *huno* comes from *\*käwno* (cf. *nägro*), how do we explain the *o* in the nonpast (from a putative *\*yəkäwn*), identical with the vowel of the past. On the other hand, assuming that the contraction took place when there had been *\*käwwänä*, *\*yəkäwwən*, *\*käwno*, we may state the following possible changes: *-äww-* (with gemination) → *-o-*, whereas *-äw-* → *-u-*. This is, of course, no proof of absolute certainty, but it is plausible to a certain extent and makes the originality of gemination a little more plausible. Polotsky did not introduce the term 'pseudo-gerundive', for the *səbirtä*-form. In fact, he branded 'gerundive' as a Gallicism, and tentatively called *səbirtä*- a 'gerund' (see his *Notes on Gurage Grammar*, p. 41). Leslau presents the only valid objection of the whole discussion here: the palatalizing effect of *i* is regressive in Ethiopian, thus positing an original *\*säbir* 'cannot bring about a palatalization of the last radical'. I am sorry I did not have a chance to explain that I assume that this form underwent the ANALOGICAL influence of the jussive form, which also introduced patterns like *nəheṣtä*- (jussive *-nkäṣ*), which has a palatalized feminine exerting a further influence on the fate of the palatal in the *səbirtä*-form. Since the Colloquium I have written an entire paper on this topic.—My point was precisely that Muher has 'double allegiance', as a *tt*-language genetically, but a Northern Gurage language through later developments.—Ennemor is not Central, but Peripheral Western Gurage. It does have a definite future in *-kwe/ -de/ -te*. Central and Peripheral Western Gurage languages are the only Ethiopian Semitic languages to have a true present distinct from the future, and vice-versa. Isn't this a clear isogloss?—In my notes I find *bäṛna*, with a glottal stop, for 'eat'. Of course, there had been a metathesis from *bänṛa* as in Ennemor. This is not in contradiction with Leslau's data who gives

*bāt'na* (JNES 30, 223) with a curious raised *t* which corresponds to what I heard as a glottal stop. Thus, this is a matter of phonetic interpretation. I do not think that anthropologists need any warning against linguistics. They have their criteria for their classifications, and if they feel like talking about linguistic classification, they do have to rely on what linguists have said. And this warning, literally taken, would also apply to Leslau's own classification. As far as the documentation supporting classification is concerned, Leslau's own work has gaps in this respect. For instance, we have no way to find out why Leslau considers Muher, Masqan and Gogot a 'possible subgroup' of Western Gurage. In reply to Professor Petráček, in a linguistic system 'every element is important' indeed, but here we are dealing with genetic reconstruction, not with synchrony. The repetition of the last radical as a plural forming process is found only in the southernmost Ethiopian languages, nowhere else, so that its being 'inherited' from proto-Semitic is unlikely. The double negation is indeed attested in many languages, including French *ne ... pas*. My point was that selecting a topicalizing particle *-mm* for the suffixal part of the negation element (in main verbs only) is due to Cushitic influence. I do agree with Dr. Fenton that no evidence for *iqattal* in Canaanite and Aramaic has been yet found, the attempts at finding it are not convincing. Yet I still consider this form proto-Semitic for a number of reasons. First of all, it is quite normal in various language families that the languages of the periphery preserve archaic features given up by the centre. Secondly, it would be hard to explain how *iqattVl* was created INDEPENDENTLY at the two extremities of the Semitic domain, why it was created, how come they are so similar (cf. Berber!), what they come from. The explanation that the gemination arose as a compensation for the loss of the final *-u* is quite weak. There is here a very general principle to be applied. When we have two genetically related systems, one richer than the other, unless we find obvious motivation for the enrichment of the richer system (taking the poorer one as basic), we have to assume that the richer one is original and the poorer one results from simplification. And in the case of Central Semitic (Arabic, Canaanite and Aramaic), we may even find a justification for the elimination of an older *\*iqattVl*, in answer to Polotsky's question in the Speiser book (1964): the reorganization of the prefix-vowel system, originally basic *y-i-qattVl* and geminate *y-u-qattil*, as a consequence of the development of Barth's law and the internal passive in *yu-*, made the distinction between the basic and geminate stems hard to maintain. The prefix-vowel was no longer characteristic of a given stem. This led to the decadence of the geminated imperfect form of the basic stem.





## WHAT IS A SEMITIC ETHIOPIAN LANGUAGE?\*

WOLF LESLAU

From a descriptive point of view, there is a North Ethiopian group of languages that includes Ge'ez, Tigre, and Tigrinya; and a South Ethiopian group that includes Amharic, Argobba, Gafat, Harari, and Gurage.

The present paper deals with phonological and morphological features that are found only in Semitic Ethiopic, to the exclusion of the other Semitic languages. These features are either innovations within Ethiopic or brought about by the influence of Cushitic. I consider a specific Ethiopian feature the one that is found in all the Ethiopian languages of the North Ethiopian and the South Ethiopian group or in the majority of languages of either the North Ethiopian or the South Ethiopian group. These features are:

Labiovelars *g<sup>w</sup>*, *k<sup>w</sup>*, *q<sup>w</sup>*, *h<sup>w</sup>*, (*h<sup>w</sup>*) in all the languages except in Tigre, Harari and some East Gurage dialects.

Prepalatals *ǧ*, *ʒ*, *š*, *č*, *ć*, *ň* in all the languages except in Ge'ez.

The glottalized pronunciation of *k* (*q*), *č*, *t*, *š*, *p*.

New sounds: *p* and *p*.

A sex-distinguishing word. Example: Tigre *tāb'at* for 'male', *'anəst* for 'female'.

Special elements for the verb 'to be' for the expression of existence (Amharic *allä*) and for the expression of quality or identity (Amh. *nāw*).

In the verb, type B corresponding morphologically to the so-called 2nd form of Arabic or the *pi'el* of Hebrew, and type C corresponding to the so-called 3rd form of Arabic are, as a rule, no longer derived stems, but lexical items.

The imperfect has the pattern *yənäg(g)ər* and is distinct from the jussive pattern *yəngər* (*yəlbäs*).

The active participle has the pattern *qāṭali* in the majority of the languages.

A special morpheme for the expression of the causative of the intransitive and of the transitive verbs: for the intransitives it is (')*a-*, for the transitive it is (')*at-* or *as-*. The North Ethiopian situation is still to be examined.

A causative of reciprocity. Example: Tigrinya *'annaxäsä* (for *'atnaxäsä*) or *'annäxaxäsä* (for *'atnäxaxäsä*) 'cause that they bite one another'.

\* Résumé.

All the languages except Ge'ez form a reduplicative stem *nāgag(g)ärä* for the expression of an intensive, frequentative or attenuative action.

A series of verbs with the prefix *'ən-* or *tän-* as lexical items.

Composite verbs consisting of a fixed element with the verb 'to say' (Amh. *zamm alä* 'be quiet').

#### DISCUSSION

LESLAU: I do not want to add anything more to my paper. The reason for these mental gymnastics was basically to invite my other colleagues in the field to see which features can be either innovatory or be characteristic of one or another Semitic, and perhaps Hamito-Semitic, language—to see which kind of features can be created in a specific language; as, for instance, in the case of Ethiopic, even though it belongs to Semitic yet it has features of its own which are not found in the other Semitic languages. Now there are reasons for it, of course, possibly simply an innovation within the language group as is the case in many languages, or we may suspect an influence of a neighbouring language, in this case Cushitic. So it would be of interest to see also in other Semitic languages—perhaps Akkadian, where a possible influence of Sumerian may come in—which are the features that can be isolated and innovated within the specific languages and, so to speak, play around with them to see their relative importance within the group and in general what can be innovated in a language, even though it belongs to a specific group. I don't think I have to go over the specific points since it is all there in the *résumé*. The only thing that I would add concerns the rather revolutionary finding of Dr. Johnstone's paper, namely the presence of a series of ejectives in Modern South Arabian, since it may possibly throw out one of the features, the glottalized consonants. So far we thought that Ethiopic was the only language that had a glottalized series, *k'*, *t'*, *p'*, *s'* and so on. Now, I won't say a newcomer comes—modern South Arabian is an old language in the field—but it is for the first time that we hear that modern South Arabian has also glottalized consonants, and of course this poses once more the problem of possible relationship between South Arabic and Ethiopic.

ULLENDORFF: Perhaps I might refer to the last point first, about glottalized ejectives. They are not quite so unknown in Semitic languages, after all we must have had it in Cairene Arabic because you cannot possibly otherwise explain the existence of *'amar* 'moon' instead of *qamar*, and this is not explicable from the normal pronunciation of Arabic *qāf* but only from a glottalized ejective pronunciation. I am sure we are both in agreement about this. I think Professor Leslau will also probably agree that none of these features, or almost none of them, in isolation can be considered as characterizing Semitic Ethiopic. It is only the sum total of them taken together that can be considered in this light. Because, if you look at the prepalatal series, two of these occur in almost all Semitic languages; the glottalized ejective

pronunciation I have already discussed. So it is only the combination of all these features together which makes up the essential character of Ethiopian Semitic. But this does seem to me to be the right approach, that we select a number of features which do not occur prominently in the other languages of the group, and their total effect then will combine to contrast with other language groups.

KAYE: Regarding glottalization or ejectives I would be very interested to know which is more original, the pharyngalized type of Arabic or the glottalized type of Ethiopic.

VYCICHL: Khârga Oasis has a glottalized *q*—*el-qulla* is called *el-q'ull*.



## ON ROOT STRUCTURE IN PROTO-SEMITIC

I. M. DIAKONOFF

Soon after publishing my book *Semito-Hamitic Languages* (1965) I arrived at the conclusion that the chapter on root structure was quite inadequate, containing numerous slips and not giving a true picture of the situation. This led me to undertake a new and more thorough study of the problem; in its complete form, including all illustrations etc., this is published in the 1970 number of *Archiv Orientální*, which should be consulted by all who wish to be informed of the more detailed argumentation in favour of the ideas here presented.<sup>1</sup>

The general results of my study, stated more concisely, are as follows (although in the following we will keep in view the possibilities of an approach to the reconstruction of Common Semito-Hamitic phenomena, our immediate task will none the less be a study of Common Semitic material only; this task will, naturally enough, include not so much the discovery of new facts as a systematic interpretation of facts already ascertained, mostly by other scholars):

It is well known that the Semitic roots can be neatly subdivided into verbal and primary nominal roots. In the verbal roots the vocalism has a functional role and changes according to certain apophonic laws which have lately been studied by Kuryłowicz (1958, 1961), while the vocalism of the primary nominal roots is, in the proto-Semitic prototype, a stable part of the root morpheme itself. This stability of the vocalism of the root morpheme in non-derivative nouns is completely preserved in Akkadian and can be reliably reconstructed in the Northern Central (North-Western) Semitic languages, where the variations in vocalism depending upon status and number of the noun are secondary. The stability is less obvious in the Southern Semitic languages, in the first place because of the profuse growth of the system of *pluralis fractus*, but also because of the nearly unlimited freedom with which new derivative verbs are formed from nouns and secondary nouns derived from verbal roots. In addition the stability of the primary nominal vocalism is some-

<sup>1</sup> A paper on this topic was read at the session of the Semitic Circle in Prague in January 1967 (see *Archiv Orient.*, 36, 1968, 471) and published completely in *Archiv Orient.*, 38 (Diakonoff 1970). The present paper is a shorter version of the same.

what disturbed by the wide-spread habit of forming various word-base patterns for diminutive nouns by changing the vocalism of the root morpheme not only in derivative but also in primary nouns, a device also observed in Northern Semitic languages.

Nevertheless, the Southern Semitic languages preserve at least one feature typical of the proto-Semitic non-derivative nominal root morpheme, and perhaps its most important feature, namely the non-functional character of the vocalism (in the singular). In other words, while the difference between the vocalism in the noun patterns *fa'l-*, *fi'l-*, *fā'il-*, *fa'il-*, *fa'ūl-* derived from verbal roots corresponds to a difference in their semantic and/or grammatical function, the difference in the vocalism of various primary nouns has no connection with their semantic function.

A system of six vowel phonemes (*a*, *i*, *u*, *ā*, *ī*, *ū*) can, as is well known, be reliably reconstructed for proto-Semitic. In traditional Semitology it was thought that these vowels are never a part of the root in Semitic, the root being reconstructed as consisting of consonants only. In the last decades, however, this point of view is being more and more abandoned. At present it seems hardly possible to insist upon the vowels being no part of the root in pronouns, numerals, and in fact, in primary nouns generally; moreover many Semitologists are now inclined to consider it possible for a vowel even to be part of a verbal root, although at present there also exist adherents of the theory which asserts that vowels are not and never have been part of ANY Semitic root morpheme (Gazov-Ginzberg 1965, 90 sqq.).

Obviously, the answer to the problem of what is the root morpheme to be separated out of a word-base in a Semitic language depends upon the analysis of the morphonological structure of the word in which alone the root morpheme can exist. The notion of 'root' is certainly no more than an abstraction with no real content except under the conditions existing in real words.

Let us begin by considering the proto-Semitic nominal morphemes as a closed system of facts, and by attempting to deduce the structural principles of the formation of nominal root morphemes exclusively from phenomena existing inside this system.

Of course, we have not set up a complete list of proto-Semitic nominal roots. For our present purpose we can limit ourselves to the lists of the more important nominal roots, mostly attested in all four groups of Semitic languages, which have been published by Bergsträsser (1928, 181 sqq.), Gazov-Ginzberg (1965, 92-94), and myself (1965, 32, 42-52, 56). These lists have been classified in accordance with the phonological structure patterns which will be established below.

Investigating the Semitic primary nominal roots as a self-contained system, the first thing which we discover is that here there exists a structure of vocalism totally different from the one reconstructed for proto-Semitic as a whole—or, to be more exact, for the diachronic level immediately preceding the division of proto-Semitic into four separate dialects. As is well known, and as has already been mentioned here, six vowel phonemes can be reconstructed for proto-Semitic: *a*, *i*, *u*, *ā*, *ī*, *ū*. The situation is different in the sub-system of primary nominal root morphemes:

for this sub-system certain specific phonological rules can be formulated, namely the following:

(1) With the exception of a few cases where vowel length appears to be secondary (e.g. *\*tūm*- 'garlic', *\*mā'i*- 'water', *\*bāb*- 'door'), long vowels are absent in the proto-Semitic primary nominal root morphemes.

(2) While in the vowel scheme as reconstructed for proto-Semitic generally, the vowel *u* appears as a separate phoneme, in the sub-system of proto-Semitic primary nominal root morphemes *u* is an allophone of the *i* phoneme appearing in contact with the labial consonants *b*, *p*, *m*, sometimes also with *g*, *q*, *k* (when  $< *g^w$ ,  $*q^w$ ,  $*k^w$ ) and, in a few cases, in contact with the (labialized?) glottal stop  $'$ . Thus in the sub-system under consideration there seem to be only two vowel phonemes, *a* and *i/u*. We may add that in unstressed(?) position the vowel *a* lost its definite quality and could be reflected, in the different language groups, either as *a* or as *i*. It is not impossible that the difference between the prototypes of the *a* and *i* phonemes when under stress, was one of pitch, e.g., that  $*a < *ā$  and  $*i < *ī$ , while their articulatory characteristics might have been identical. However, in our reconstruction of the proto-vocalism in the sub-system of the Semitic primary nominal root morphemes we will only use the  $*ā$  symbol for the prototype of *i/u*, without insisting on this notation having any definite phonetic significance, while retaining the *a* symbol for the prototype of the historically attested *a* vowel.

(3) Lastly, a third phonetic rule obtains for the sub-system of proto-Semitic nominal root morphemes, with reference to the phonemes  $'$ , *i*, *u*, *l*, *r*, *m*, *n*. A primary nominal root morpheme may contain either two or three different consonants and either one or two vowels, but if it contains three consonants and one vowel, then:

- (a) the morpheme invariably has the structure  $C_1VC_2C_3$ ;
- (b) either  $C_2$  or  $C_3$  is a phoneme of the group  $'$ , *i*, *u*, *l*, *r*, *m*, *n*;
- (c) if it is  $C_2$  that is a phoneme of this group, then  $V = a$ .

This leads to the supposition that sonants of the proto-Indo-European type are to be reconstructed here, i.e. phonemes which at a certain stage in the development of the language could function both as syllabics (vocalic) and as non-syllabics (consonantal). The following rule may in this case be formulated (a sonant being symbolized by S, a 'pure' vowel by V, and a non-sonorant consonant by C):

- in an  $*SV$  sequence the sonant is non-syllabic:  $'$ , *i*, *u*, *l*, *m*, *n*, *r*;
- in an  $*SC$  sequence the sonant is syllabic:  $a'$ ,  $a_i$ ,  $a_u$ ,  $al$ ,  $am$ ,  $an$ ,  $ar$  (i.e. it resolves into a diphthong).

The sequences  $*\#VS$  and  $*\#SC$  cannot occur. This results from the general Semito-Hamitic rule that a syllable cannot have a vocalic *Anlaut*. Nor can the sequence

\*CSV or \*SCC occur inside one syllable, a fact which results from another general Semito-Hamitic rule of syllable formation, namely that no syllable can begin with two consonants or be closed by two consonants (the sequence \*CSV and \*SCC being equivalent to \*CCV and \*VCC). If, however, the sequence in question is divided between two syllables, then the rules stated above obtain: i.e. the syllable sequences \*CVC|SV, \*CVC|CV are possible, the sequences \*#VC|SV and \*#SC|CV are impossible. As to the sequences CS# and CSC#, they constitute a case apart which will be discussed separately below. For the present, it is sufficient to state that an \*-CSC- sequence is ruled out even if the division of the syllables is \*CSC|CV.

The system of vocalism just described being obviously not in accordance with the situation reconstructed for proto-Semitic at the diachronic level immediately preceding its division into dialects, we must assume that our inner reconstruction (back from proto-Semitic) reflects a certain earlier stage of linguistic development. However, as the six-vowel system can reliably be reconstructed not only for proto-Semitic but also for proto-Libyco-Berber and is, therefore, probably proto-Semitic-Hamitic, we have still to go a step further and to admit that our reconstructed scheme of vocalism as suggested for the sub-system of primary nominal root morphemes, i.e. seven sonants and two vowels proper, refers to a still earlier diachronic level which we may conventionally call 'pre-proto-Semitic-Hamitic'. It is in any case not later than proto-Semitic-Hamitic, because there is evidence that a number of primary nouns belonging to the common Semito-Hamitic vocabulary show a vocalism identical to that in the Common Semitic primary nouns sub-system. But as the Common Semito-Hamitic roots have not been sufficiently investigated, we shall also limit ourselves in the following to illustrations taken exclusively from Semitic. It must, however, be kept in mind that the picture we are reconstructing is considerably older than proto-Semitic.

In order to make the following more comprehensible, one should start from the premise stated above that a root morpheme never did and never could exist as such outside of a word as a fact of reality. Therefore, in order to study root morphemes, one should consider (1) the laws of syllable formation, (2) the laws of syllable contacts.

Judging from the unanimous evidence of the Semitic languages (which is, on the whole, supported by the evidence of other Semito-Hamitic languages in so far these New Stage languages allow of making a reconstruction of the situation which must have existed at the Ancient and Middle Stages), the only possible types of syllable were either -CV- or -CVC-; we have every reason to postulate this situation already for proto-Semitic-Hamitic. Therefore, we may assume a similar situation to have existed also at the 'pre-proto-Semitic-Hamitic' stage with which we are concerned, although in this case such an assumption is to be modified to suit the specific system of vocalism peculiar to this stage. In other words, we will assume that at the diachronic level in question not only were possible syllables of the -CV- and -CVC- types but also equivalent syllable types, where either the consonant or the vowel



was replaced by a sonant. It will be shown that this assumption allows the reconstruction of a consistent theory of nominal root formation.

According to the above formulated rule (a sonant in contact with a vowel is a consonant, a sonant in contact with a consonant is a vowel), only such combinations of C, S and V are possible which exclude the clustering of either two consonants or two vowels in one syllable; i.e. a syllable always begins with no more and no less than one consonant and never ends in two consonants. This means that only the following sequences are admitted in one syllable:

CV, and the equivalent sequences SV, CS, and SS;  
CVC, and the equivalent sequences CVS, CSC, CSS, SVS, SSC, and SSS,  
other sequences being impossible inside one syllable.

Thus a syllable of the \*SC type is impossible because here S = V and cannot begin a syllable, and a syllable of the \*CSV type is impossible because here S = C (or = V), and a cluster of either two consonants or two vowels in one syllable is forbidden, etc.

This, however, is not enough, because not only the rules of syllable formation but also the rules of syllable contacts must be taken into consideration. Starting from the premise that two consonants cannot cluster inside one syllable and thus can neither begin nor close a syllable, we may formulate the following rule of syllable contacts: not more than two consonants may cluster at a syllable boundary. Another rule, the reasons for which will appear more clearly below, is that a sonant cannot precede or follow a cluster of two non-sonorant consonants; that is, sequences of the types \*CSC+CV, \*CSC+CVC, \*CSC+CSC, \*CVC+CS, \*CVC+CSC are forbidden.

As I have attempted to show elsewhere (1965, 57 sqq.; 1967, 213), a proto-Semitic noun could exist either in a zero case (this zero, or absolute, case was used when the noun had no syntactic connections or when it was a predicate and, at some very early stage, apparently also when the noun was the subject of a state),<sup>2</sup> or it could

<sup>2</sup> This may be deduced from the observation that the personal affixes of the verbal form expressing state—the stative, as in Akkadian, of which the ‘new perfective’ of the other Semitic languages is a later development; the qualitative-stative, also called ‘pseudo-participle’, in Egyptian, in Kabyle (Berber), in Musgu (Chado-Hamitic) etc.—have developed from the direct case of the personal pronouns while the actor affixes of the verbal forms expressing action cannot be related to such pronouns; from this it follows that the subject of state was originally expressed by the direct case while the subject of action was expressed by a quasi-indirect, namely the ergative, case (in ergative languages the case of the subject of the state is usually expressed by zero inflection of the noun). The most ancient Semito-Hamitic languages have the following typical features in common with languages characterized by the ergative construction of the sentence: (1) the existence of a formal contrast between the categories ‘action : state’ and the absence of formal contrast between the categories ‘active : passive’ (the so-called Semito-Hamitic passive is always of secondary origin and usually expresses the impersonal character of the action or the state, not necessarily being in contrast to the active voice; it is not an expression of the point of view of the logical object of action as opposed

exist with a vocalic suffixal inflection. Thus Old Akkadian had, among others, an *-u-* inflection which expressed on the one hand the locative and on the other the nominative (probably originally the ergative) case, and an *-i-* inflection expressing the genitive (which was probably originally a more general 'subjoined' relative case). Having postulated that, at the diachronic level which is being investigated in the present paper, the vowels *i* and *u* coincided in a proto-phoneme  $*a$ , we must assume that at this level there was no differentiation between the locative-ergative morph and the relative morph; this, possibly, is corroborated by the fact that in Egyptian we encounter a possessive construction of the transitive sentence (which is a variant of the ergative construction with the sole difference that the ergative case of the subject of action coincides formally with the genitive). There are also other indirect indications of the absence of differentiation between *i* and *u* in Egyptian at an early stage. But be it as it may, we must certainly assume the existence both of zero inflection and of vocalic inflection in proto-Semito-Hamitic.

This means that, since the root morpheme could exist only inside a REAL noun, its structure must have been such as to make both a zero inflection and a vocalic inflection of the noun possible. From the first condition it follows that any root morpheme must either consist of one full syllable or end in a full syllable; from the second condition it follows that the structure of a monosyllabic root morpheme (or of the second syllable of a bisyllabic root morpheme) must make a vocalic inflection possible without violating the rules of syllable contacts, i.e., the adding of a vowel must result in two permitted syllables, viz. CV|C+V (or its equivalents: CS|C+V, SS|C+V, SV|S+V, SV|C+V, SV|S+V, CS|S+V, SS|S+V). Thus, a root morpheme of the type  $*C_1VC_2C_3$  is not allowed because, although it does not contradict the rules of syllable contacts in the case of a vocalic inflection ( $*C_1VC_2|C_3+V$ ), it does contradict the rules of syllable formation in the case of zero inflection.<sup>3</sup>

to the active voice expressing the point of view of its logical subject); (2) formal morphological coincidence between the subject case (probably this was originally the actor case only) and the locative (in Akkadian) or the genitive (thus probably in Egyptian); (3) the existence in dynamic (transitive) verbal forms not only of a personal affix expressing the actor, but also of personal affixes expressing the object (i.e., the subject of the resulting state); moreover (4) there exist some indications that the object case (accusative) originally coincided with the zero case (in the noun: Diakonoff 1965, 58), or with the direct case (in the pronouns; this direct case is termed nominative; however, the pronouns of this form are not used directly to express the subject of an action, but only for intonational emphasis: Diakonoff 1965, 70-73, 86). In languages with an ergative construction of the sentence the object of action (or, better, the subject of the state resulting from the action) is expressed in the same way as the subject of a state in general. While in Semitic the connection between the direct case of the pronoun and the subject of action is rather loose—cf. a construction like *u šū imqut-su ḥattu* 'and he, a fright fell upon him',—in Egyptian the corresponding form of pronoun is not used for the subject of action at all.

<sup>3</sup> This means that all Semitic nouns of the pattern  $C_1VC_2C_3$ , if  $C_2$  or  $C_3$  is not a sonant, must be classified as secondary formations (i.e. as derived from verbal roots) and originating at a diachronic level later than the one at which the rules stated above obtained. The same is valid with reference to nouns of the pattern  $C_1i/uSC_2$ , because in this combination S would, according to the same rules,

Neither is a root morpheme of the type \*CV allowed, because according to the rules of syllable formation and syllable contacts a sequence \*CV+V is not permitted inside one syllable, since a cluster of two vowels is forbidden; nor is such a sequence permitted astride of a syllable boundary, because no syllable can begin with a vowel.

However, an analysis of the existent root morphemes shows that a root morpheme of the pattern CS was possible, apparently because in this case the syllable boundary would pass THROUGH the sonant, owing to its double (vocalic and consonantal) nature:

CS|  
|S+V, e.g. *dm* 'blood' > *dam-#*  
*da|m-u.*

The sequence CS# was thus not completely identical with the sequence CV#, the sonant having in this case a double function, both as a vowel and as a consonant. This helps to make it clear why, although a syllable of the pattern CSC (or its equivalent) is permitted (also as root morpheme), a bisyllabic root morpheme of the pattern CSC+CVC, CVC+CSC, CVC+CS and its equivalents are impossible, being inconsistent with the rule forbidding the clustering of more than two consonants at syllable boundary.

To sum up, a monosyllabic primary root morpheme can include: (1) two phonemes, the first being either a non-sonorant consonant or a sonant, the second necessarily a sonant, or (2) three phonemes, the first and third being either a non-sonorant consonant or a sonant and the second either a vowel or a sonant: (1) CS (or with S for C), (2) C<sub>1</sub>VC<sub>2</sub> (or C<sub>1</sub>SC<sub>2</sub> or, in both cases, with S for C<sub>1</sub> and/or for C<sub>2</sub>).

A root morpheme may also be bisyllabic. In this case both its syllables must conform to the same rules and also to the rules of syllable contacts, i.e. a sonant must not precede or follow two consonants in contact over a syllable boundary—or, in other words, if the first syllable ends in a vowel or in a syllabic sonant, then the next syllable may be either of the type CS or of the type CVC, but if the first syllable ends in a consonant or in a non-syllabic sonant, then in neither of the syllables can the vowel be replaced by a sonant. Thus, the only possible patterns are:

(3) CVCS (and CSCS),

because CV|CS and CVC|S+V, CS|CS and CS|CS|  
|S+V } are not forbidden sequences;

(4) CVCVS (and CSCVC),

because CV|CVC and CV|CV|C+V, CS|CVC and CS|CV|C+V are not forbidden sequences;

be equivalent either to a vowel, being in contact with a consonant, or to a consonant, being in contact with a vowel; but this means that the sequence C<sub>1</sub>*i*/uSC<sub>2</sub> cannot represent a primary nominal root morpheme since it does not constitute either one full syllable or two full syllables of a permitted type.

## (5) CVCCVC,

because CVC|CVC and CVC|CV|C+V are not forbidden sequences.

However, the pattern \*CVCV is impossible because the sequence \*CV|CV+V is forbidden; also \*CVCCS is impossible, because even if we write  $\left. \begin{array}{c} *CVC|CS| \\ |S+V \end{array} \right\}$  the sequence is forbidden, because the sonant S in its role of consonant follows the cluster -CC-. For the same reason, are also forbidden the sequences \*CVC|CSC, \*CSC|CVC, \*CVC|CSS, \*SSC|CSC, \*CSS|CSC, \*CSS|CSS etc.

These five permitted types of nominal root morphemes have their equivalent patterns: in the types (2), (3), and (4), V<sub>1</sub> can be replaced by S, and in all five types any C may be replaced by S.

There is one more type of primary nominal root morpheme, which can be considered to be an allomorph of the type (2). Namely, with the addition of a vocal inflection the last consonant in the CVC pattern is doubled: \*C<sub>1</sub>VC<sub>2</sub>+V = C<sub>1</sub>VC<sub>2</sub>|C<sub>2</sub>V. Of course, this phenomenon cannot be observed if the vowel of the root morpheme is a syllabic sonant, in accordance with the rule forbidding contact between a sonant and a cluster of consonants at syllable boundary (the sequence \*CSC|CV is impossible). For the sake of simplicity, we will denote the type C<sub>1</sub>VC<sub>2</sub>(C<sub>2</sub>) as type (2a) CVC̣.

The word-base of the primary nouns in the historically attested Semitic languages can also include word-formative suffix morphs:

-ām-, -ān-, the so-called 'individualizing' suffix;

-(a)b-, suffix of nouns denoting harmful animals;<sup>4</sup>

-(a)r-/-(a)l-, var. -ār-/āl-, suffix of nouns denoting animals used in production;<sup>4</sup>

-(a)t, suffix denoting socially passive objects(?);

also the possessive suffix -i- (*nisbah*) and, possibly, a few more. In analyzing the structure of the primary nominal root morpheme these morphs must not be regarded as parts of the root.

We will now proceed to state the arguments in favour of the rules indicated above and, at the same time, attempt to find out the reasons for certain apparent anomalies in the formation of Semitic primary nominal roots. First of all, we will dwell upon the three phonetic rules stated above as specifically characteristic of the sub-system of primary root morphemes.

(1) *The absence of vowel length.* Examples of vowel length in primary nouns are very few and in all cases it is apparently of secondary origin.

<sup>4</sup> In postulating these suffixes we follow the observations of the late Jušmánov (unpublished); cf. Diakonoff (1967), 4, 210.

(2) *Secondary differentiation of i:u in non-derivative nominal roots.* In the material I have reviewed there are almost no cases where the *u* vowel is attested in the noun in question in all of the Semitic language groups and does not alternate with the *i* vowel at least in some of them. It is common knowledge that, even in verbal nouns belonging to a much later diachronic level, noun patterns with *i* and with *u* or with *ī* and with *ū* frequently alternate without essential change in the semantics of the pattern.

In the sphere of primary nominal roots, in all examples which we have collected the vowel *u* (nearly always in alternation with *i*) is attested only in the following sequences: (a) after *b*, *p*, *m*, (b) after *g*, *q*, *k* (apparently < proto-Semito-Hamitic *\*g<sup>w</sup>*, *\*q<sup>w</sup>*, *\*k<sup>w</sup>*), (c) before *p*, *b*, *m*, (d) in a few cases after ' (only in roots of the patterns HVCS and possibly HSC).<sup>5</sup> This last group (d) will be treated below in another connection. As to the groups (a), (b), and (c), at first it seems that the distribution of labialized and non-labialized forms among the different Semitic languages is haphazard; it can, however, be surmised that this impression is due to the scarcity of examples which can be used for its verification,<sup>6</sup> and also to our inadequate knowledge of the real history of the Semitic languages and cultures at the prehistoric level. Still, the labialized and non-labialized forms can be predicted, not with a hundred per cent certainty, but at any rate with a high degree of probability.

Apart from some inconsistencies, probably to be explained as due to inter-dialectal borrowings or to some special conditions of phonetic surroundings which cannot be identified because of the scarcity of examples, there are certain rules governing the distribution of the reflexes of *\*ə* in contact with labials and labialized consonants:

*\*bə-*, *\*g<sup>w</sup>ə-*, *\*q<sup>w</sup>ə-* > Akkad. *bu-*, *gu-*, *qu-*; Hebr. *bu-*, *gu-*, *qu-* (but with a considerable influence of an *i* dialect); Aram. inconsistent(?); Arab. certainly *bi-*.

*\*k<sup>w</sup>ə-*, *\*mə-* > probably *ku-*, *mu-* in all languages (Ethiop. *k<sup>w</sup>ə-*).

*\*-əb-* > Akkad. *-ib-*, Hebr. *-ib-*, Aram. *-ib-*, Arab. *-ub-*.

*\*-əp-* > Akkad. *-up-*, Hebr. *-ip-*, Aram. *-ip-*, Arab. *-uf-* (and *-if-*).

*\*-əm-* > Akkad. *-um-*, Hebr. *-im-*, Aram. *-im-*, Arab. *-um-*.

The number of examples is not sufficient, but even those available prove clearly the allophonic character of *u* in the sub-system of primary nominal root morphemes.

Let us now turn to the postulated specific character of the *a* vowel in bisyllabic roots and word-bases. Here we shall analyze the ancient bases with the word-formative suffixes *-āl-*, *-ām-*, *-ān-*, *-ār-*, as well as the primary roots of the patterns CV|CVC- and

<sup>5</sup> Nouns with a *u* vocalism but not belonging to the above categories are either derived from verbs, or of non-Semitic origin; the origin of *kulb-ab-* 'ant', attested only in Akkadian, is doubtful, in spite of the ancient Semitic suffix *-ab-* (< *\*k<sup>w</sup>lb-ab-*, or borrowed?).

<sup>6</sup> Which makes it impossible to take into account the influence of the positional surroundings of the phoneme with sufficient precision and with adequate differentiation of possible influences.

CV|CS- (CV|CV|C+V and CVC|S+V). We must note that the specific phonotactic conditions reigning in Akkadian and Aramaic do not always allow the distinguishing of the patterns CVCVC and CVCC, and sometimes it is also difficult to decide whether the original semantics of the root is verbal or nominal; naturally if in a  $C_1VC_2C_3$  pattern neither  $C_2$  nor  $C_3$  is a sonant or if the sequence -VC<sub>2</sub>- is equal to -əS-, then there should be no doubt as to the verbal origin of the noun in question.

First we must separate the group of root morphemes in which either  $C_1$  or  $C_2$  is one of the phonemes ' or *h*; these, as is well known, tend to give an *a* colouring to the neighbouring vowel.<sup>7</sup>

Then, apparently, a group of roots beginning with the sequence H (> ') + unstable vowel ('*a*-, '*i*-, '*u*-) should be separated. It has so far not been possible to explain the origin of this group; it may be connected with a weakening of the initial hamzah, and with the resulting reduction of the following vowel under the conditions of an emerging phrase-contraction of the sandhi-type? A more probable explanation might lie in the assumption of a labialized laryngal, a phenomenon encountered in proto-Indo-European.

After the exclusion of these groups of nominal root morphemes, all the other primary nominal root morphemes of the patterns CV|CVC- and CVC|S-,<sup>8</sup> as well as the roots with the ancient word-formational morphs -*āl*-, -*ām*-, -*ān*- and -*ār*- may be subdivided into two more groups: (a) those with a stable vowel in the first syllable, and (b) those with an unstable vowel in the first syllable. Cases of vacillation between *u* and *i* are classified with the first group, because *u* is regarded as an allophone.

It will be noted that the stable vowel of the first syllable is usually \**a* = *i/u*. However, in contact with \**H* = ' also a secondary(?) \**a* may appear as a stable vowel. In the following examples the vowel is unstable (the distribution between the individual languages being illustrated in the form of a table):

	Akkad.	Hebr.	Aram.	Ethiop.	Arab.
* <i>g<sup>w</sup>ar-(ān-)</i> <sup>9</sup>	i	a	—	—	i
* <i>laš-(ān-)</i>	i	a	i <sup>10</sup>	i	i

<sup>7</sup> The sequence -CS- at morpheme boundary, the suffix beginning with -C, seems to result either in -CaS| + C- or, if there are favourable conditions as to the rules of syllable contact, also in -C|Sa-|C-; perhaps stress conditions could play a role.

<sup>8</sup> In Arabic, as well as in Hebrew, examples of expansion of the pattern CVCC- (including <\*CSC-, \*CVCS-) > CVCVC- and, conversely, a contraction of the pattern CVCVC- > CVCC- are common; however, the secondary form usually exists as a variant of the main form.

<sup>9</sup> The root is here in the  $C_1aC_2$  ( $C_2 = S$ ) degree, not \* $C_1S$ , because in connection with the suffix a forbidden biconsonantal anlaut would arise; at least, this must be the explanation of the form unless we decide that the suffixation is later than the process \* $g^w\gamma$ - > *gar*-. Cf. the root variant *g<sup>w</sup>ār*- (or a secondary verbal root \**grr*) in the Hebr. *gēr-ā* 'cud', and the root variant \* $g^w\gamma$  in Hebr. *gargār-āt*, Aram. *gargar-t-ā*, *gaggār-t-ā* 'neck, throat'.

<sup>10</sup> The reduplication is here probably the result of a rhythmical assimilation of the pattern \* $C_2C$  to the pattern \* $C_2CC$ , -*ān*- still being felt as a suffix not belonging to the root.

	Akkad.	Hebr.	Aram.	Ethiop.	Arab.
* <i>dakar-</i>	i	a	i	—	a
* <i>danab-</i>	i	a	i	a	a
* <i>daqan-</i>	i	a	i, a	—	a, i
* <i>kanap-</i>	a	a	i	i	a
* <i>šakar-</i>	i	i	i	(i)	a
*( <i>baraq-</i> ) <sup>11</sup>	(i?)	a	(a?)	—	(a?)
* <i>baṭn-</i>	—	a	i	—	a
* <i>našr-</i>	a	a	i	i	a
* <i>ragl-</i>	—	a	i	—	i

This table shows clearly that Akkadian, Aramaic and Ethiopic are typical 'i dialects', and Hebrew a typical 'a dialect'. Sporadic forms, like Akkad. *kapp-*, *našr-*, Hebr. *šēḳār*, Ethiop. *zanab-*, can be explained as the result of interdialectal contacts, and Aram. *daqn-ā* (and *barq-ā*) also as the result of contamination of forms since the indeterminate form *dəqan*, *bəraq* may belong as well to *\*diqn-ā*, *\*birq-ā* as to *\*daqn-ā*, *\*barq-ā*. The picture in Arabic is more complicated; however, the rule seems to be that the first vowel is *i* if the second vowel is long but *a* if the second vowel is short. Forms like *diqan-*, *riḡl-* may be due to borrowing, perhaps from some South Arabian dialect.

The stated situation is most easily explained by the supposition that the most ancient stress lay on the second syllable (including the syllable formed by the final sonant). In these conditions *\*ə* was preserved as *i* (or, in contact with a labial or labialized consonant, *u*), while *\*a* had a tendency towards reduction which resulted, in the different dialects, either in *i* or in *a*.

(3) We shall omit illustrations to the third of the main phonetic rules obtaining for the sub-system of such morphemes: *the double (syllabic and non-syllabic) quality of the sonants 'i, u, l, m, n, r*; cf. the complete publication in *Archiv Orientalní*, 1970.

The pattern CVCCVC seems always to be the result of a compounding of roots; it is very rare, the roots belonging to it tending to be adjusted to other more frequent patterns, and the original type is often preserved in one or two languages only. The pattern is otherwise reflected irregularly.

The number of roots with other structural patterns that might claim to be primary nominal roots is very limited. But it may be expedient to dwell briefly on some

<sup>11</sup> The example is not altogether reliable. The Akkad. *berq-* may be regarded as the result of a contamination of the types CVCVC and CVCC; in Aramaic we would expect *\*berq-ā*, but irregular alternations of the patterns CaCC- and CiCC- in Aramaic are nothing out of the common owing to reasons stated below, p. 144. However, also in Phoenician do we encounter *\*barqā* (apparently feminine) instead of an expected *\*baraqā*. It is possible that all the forms in question except perhaps the Hebrew should be traced to the derivative verbal root *\*brq*, which could be an explanation why the word does not fit into the scheme of correlations for the pattern in question. Cf. also the derivative *nomen instrum.* in Ethiopic: *mabrāq*.



anomalous and doubtful forms historically attested in the individual Semitic languages.

Some processes were produced by rhythmic analogy (\**dṁ*- > \**dam*- > \**dām*-; \**mḥ*- > \**ma*'- > \**mā*'-; \**śH*- > \**śa*'- > \**śā*'-, etc.). An important circumstance which could bring about changes in the primary vocalism seems to have been the existence of cases of zero inflection. While the CVC pattern (with a 'pure' vowel) would result in CVC#, and the same happened to the CVĀ pattern, the patterns with sonants (CS, CSC, CVCS) could be subjected to certain changes. Theoretically, the patterns CS and CVCS should not cause any difficulty because S is here syllabic and, consequently, these patterns should yield such forms as CS# > CaS#, CVCS# > CVCaS#.<sup>12</sup> This is, in fact, what we observe at least in two Semitic dialects. In the Assyrian dialect of Akkadian, where the CVCS-pattern with zero inflection results in '*uzan*, '*ṣupar* etc.,<sup>13</sup> and apparently also in Aramaic at a very early stage where the corresponding forms were it seems \*'*idān*, \*'*īpar*,—whence in the historically attested period '*ēdan*, '*idn-ā*, '*ṭēpar*, '*ṭīpr-ā*,—and then also, by analogy, such forms as \*'*šamaš* > \*'*šamaš* were accompanied by such forms as '*šimš-ā* instead of the expected \*'*šamš-ā*. By the same reason \*'*baṭn*- 'womb' > '*beṭn-ā*, '*buṭn-ā* etc. The result is that in Aramaic the original patterns C<sub>1</sub>iC<sub>2</sub>C<sub>3</sub> and C<sub>1</sub>aC<sub>2</sub>C<sub>3</sub> are often reflected irregularly.

In most of the Semitic dialects, however, the patterns CVCS- with zero inflection began to acquire a vocalism analogous to the CSC pattern. In this case, a development \*CSC# > \*CaCS# was impossible for, although so long as S was a vowel the sequence \*CSC was a normal syllable and therefore could combine with both zero and vocalic inflection, as soon as it became a diphthong aS the S became non-syllabic (= consonantal) and therefore the rule forbidding two consonants at the end of a syllable came into action. This meant that CSC# had to develop into CaSaC# (also CaCaS# and by analogy even CəCəS# when the pattern was CVCS). In several instances this vocalism became normal for the word in question, even with vocalic inflections, hence the Hebrew forms \*'*buk(u)r*-, \*'*zi'(i)b*-, \*'*ri'(i)m*-, or the Arabic *zuf(u)r*-, *ša'(a)r*- etc.

<sup>12</sup> However, it seems that S# could be reflected not only as -aS# but also as -Sa. Hence, through *systemzwang*, may have emerged as a more general rule the free variation zero inflection vs.-a-inflection from which may be deduced the ending -a (parallel with zero) of the predicative form of the noun in the most ancient Akkadian material (as attested by the earliest borrowings into Sumerian, and partly in Akkadian PN), in the most ancient Amorite, and possibly also the form in -a of the Ethiopic *status constructus*. Hence also the ending -a of the stative → 'new perfective' (= \*predicative, or zero case use of the verbal noun) in Arabic, Ugaritic and Amorite as contrasted with zero in the corresponding forms in Akkadian (and Canaanite?). Hence also the -a- inflection of the accusative if, as we have suggested, the accusative was a development of one of the variants of the 'absolute' (zero) case, expressing in languages with an ergative construction of the sentence both the subject of a state in general and also the subject of a state resulting from an action (i.e. the direct object of a transitive verb). Such an 'ergative construction' seems to have preceded the historically attested 'nominative construction' of the sentence in Semitic.

<sup>13</sup> Thus also, by analogy, in derivative nouns of the patterns C<sub>1</sub>aC<sub>2</sub>C<sub>3</sub>, C<sub>1</sub>i/uC<sub>2</sub>C<sub>3</sub>.



Then phonotactic conditions existing in Akkadian and Aramaic brought about a virtual contamination of the patterns CVCVS and CVCS, as well as CaSaC and CSC. With a zero inflection one would, theoretically, expect to be able to distinguish between patterns of the types CaCiS# and CaSiC# on the one hand (probably these were usually derivative nouns) and patterns of the types CaCaS# and CaSaC# on the other the last named types corresponding also to the original CaCS- and CSC- patterns). But in practice this led to a merging of patterns (cf. e.g. Akkad. *karš-*, *karaš-* 'stomach', Aram. *kirs-ā*, *kars-ā*, *kāres#*, Ethiop. *karš* but Hebr. *kārēš*, Arab. *kariš-* and even *kirš-*; or Akkad. *malk-*, *malik-#*, Hebr. *mālāḳ* (< \**malk-*), Aram. *malk-ā*, *mālek#*, Arab. *malik-* 'king'; Akkad. *napiš-t-*, Hebr. *nāpāš* (< \**napš-*), Aram. *napš-ā*, Ethiop. *nafs*, Arab. *nafs-*). The two last words are pretty certainly derivative; generally speaking, any nominal base pattern which may be written as C<sub>1</sub>VC<sub>2</sub>C<sub>3</sub>, where VC<sub>2</sub> ≠ S and C<sub>3</sub> ≠ S, is suspicious from the point of view of the probability of its primary origin, even if the verbal root in question has not been preserved in the historically attested languages. Still more so is this the case if there does exist such a verbal root, even although traditionally the verb has been thought to be secondary and the noun to be primary.

At a very early period 're-vocalizations' of primary nominal roots to give diminutives, formed after the patterns CəCăC, CəCiC, must have appeared. The first pattern later lost its productivity and the forms in question lost their diminutive connotation, as e.g. \*'*inās-*, \*'*unās-* 'man (in contrast to animal)', and also perhaps by contrast and analogy, '*ilāh-* 'god', '*inā* 'vessel', *dubāb-* 'fly', \*'*tu'āl-* 'fox', \*'*libāb-* 'heart', etc.

Especially numerous are the anomalies in word-base formations from primary nominal roots in Arabic. This is natural because the system of internal vocalic inflection (also employed as a method of word-formation) has developed much more widely in Arabic than in other Semito-Hamitic languages. Beginning with the formation of diminutives and of forms of *pluralis fractus* (which originated in the sphere of verbal nouns, where it was a device of contrasting abstract and concrete nouns, as well as of nouns in the collective sense and in the sense of individual objects formed from the same root and with an identical basic semantic content, later overflowing into the sphere of primary nouns), the internal inflection was then used for forming nouns from verbal roots, a process which is much easier in the Southern than in the Northern Semitic languages, where the main *stirps* ('stem') of a verb is seldom formed from a nominal root. Then from new verbal roots numerous secondary nouns were formed in their turn (e.g. Arab. '*aḍīn-* 'ear'), sometimes surviving the verb from which they originated. Alongside all this some other processes distorting the original situation are observed in Arabic as well as in the other Semitic languages. Typical of Arabic is the already mentioned phenomenon of contraction of much used words of the type CəC in rapid coherent speech (*sandhi*), leading to the emergence of such forms as \*'*bin-* > *bn-* > (*i*)*bn-* etc. Following this such forms might receive, first in initial position, later in all positions in the sentence,

a prothetic inorganic 'i-, 'u- (hence the variants Arab. 'anf-, 'inf-, 'unf- 'nose'; cf. also 'ins- 'mankind' and Akkad. niš- 'people', with 'ins-ān- 'human being, man' as a *nomen unitatis*).

Lastly we must mention that in proto-Semitic and proto-Semito-Hamitic, as in all 'proto-languages', there must have also existed root variants.

Of course there remains a certain, though small, number of roots which so far defy explanation and classification, as for example the single consonant root \*p- 'mouth' (Akkad. pī-, Hebr. pā-, Aram. pūm-ā, pumm-ā, Ethiop. 'af, Arab. fū, fumm-, famm- etc.), which is incidentally Common Semito-Hamitic.

The sub-system of Semitic verbal roots and of word-bases derived from these differs from the sub-system of primary nominal roots not only typologically but also chronologically. The whole verbal sub-system certainly could not have existed as we know it before the creation of the six-vowel phonological structure, because it is precisely the vocalism, and moreover this particular system of vocalism and not the one which may be reconstructed for the sub-system of nominal roots, which is the means allowing the differentiation of the various grammatical and word-building forms inside the verbal system, including the verbal nouns. It is to this diachronic level that the mechanism of the Semitic apophony interpreted by Kuryłowicz belongs. Therefore, in spite of our disagreeing with Kuryłowicz's reconstruction on some points because some of his relative dates for certain phenomena in Semitic seem to me too late or too early in the light of the data of other Semito-Hamitic languages, we hardly need touch in this paper upon apophonic laws; instead we will attempt, by inner reconstruction, to reach an earlier conventionally speaking 'pre-proto-Semito-Hamitic' diachronic level. The task is here considerably more difficult than in the sphere of primary nouns and we will therefore limit ourselves to suggesting a preliminary outline of a solution. It should be stressed that any solution must for the moment be, and will perhaps always remain, highly hypothetical.

What is the reason for the fact that the type of the historically attested Semitic verbal root is, in principle, quite dissimilar to that of the primary nominal root? In a verbal root there may exist three consonants, none of which needs to be a sonant even if they cluster, and it is usually impossible to establish any particular vowel as belonging to the root. Although sometimes the vowel of one particular verbal form, usually that of the 'old perfective' → the Western Semitic 'imperfect', is taken to be part of the root, logically one verbal form would seem to be as good as another.<sup>14</sup>

<sup>14</sup> As I have attempted to show elsewhere (1965, 79 sqq.; 1967, 229 sqq.), the conjugated forms of the Semito-Hamitic verb, based on the semantic contrast 'imperfective : perfective' (or 'durative : punctual') and on the formal contrast 'full vocalism : contracted vocalism' appear to be as old as the system of Semito-Hamitic prefixal conjugation itself. But, if this proves to be true, then there is no reason why one should consider the vocalism of the 'old perfective' (or the Akkadian preterite) as belonging to the root more than the vocalism of the 'old imperfective' (or the Akkadian present). At the diachronic level at which this verbal system originated it is evidently correct to consider

To answer the question we should, as in the case of the primary nominal root, start from the premise that a root can exist only as part of such lexico-grammatical forms as really exist in the language.

In our first approximation we may exclude from our analysis the forms of the 'participle of action' and of the *mašdars* because these forms, as attested in the individual Semito-Hamitic languages, cannot be traced back to Common Semito-Hamitic prototypes and therefore do not belong to the proto-Semito-Hamitic diachronic level (or, even in most cases, to proto-Semitic). The same is true of the so-called passive, which also cannot be traced to the proto-Semitic and proto-Semito-Hamitic levels.

Thus, we will have to analyse only: (1) the form of the 'participle of state' (*CaCaC-*, *CaCiC-*, with the variants *CaCāC-*, *CaCiC-*, *CaCūC-*) which in its predicative use is identical with the stative (the form which preceded the 'new perfect' of the later Semitic languages) and, (2) forms connected with prefixes of the pattern CV-/SV- (e.g., *ma-* which serves to form certain derivative nouns, or *ja-* which serves to form the third person of the finite verb of action).

Let us consider the second case first. It will become apparent directly that the peculiarities of the Semito-Hamitic verbal structure are connected with the peculiarities of the Semito-Hamitic verbal inflection (namely, inflection by prefixation). We will also take it as granted that the suffix of a verbal form may be either zero or vocalic. Then, according to the rules of syllable formation formulated above, the following patterns are not forbidden for a verbal root:

- |                          |                                           |
|--------------------------|-------------------------------------------|
| (1) <i>Sa+CS (+V)</i>    | ( <i>Sa CS</i> , <i>SaC SV</i> )          |
| (2) <i>Sa+CVC (+V)</i>   | ( <i>Sa CVC</i> , <i>Sa CV CV</i> )       |
| (3) <i>Sa+CVCVC (+V)</i> | ( <i>Sa CV CVC</i> , <i>Sa CV CV CV</i> ) |
| (4) <i>Sa+CCVC (+V)</i>  | <i>SaC CVC</i> , <i>SaC CV CV</i> .       |

Equivalent variants with sonants are also possible. Thus in pattern (2) a sonant can stand both for C and for V, in pattern (3) for C and/or for one of the vowels, and in patterns (1) and (4) for C only.

We can thus make a very important statement, namely that the conditions of existence of the verbal root inside a word allow the possibility of its including three consonants, none of which is a sonant, even if two of them are in contact.

that there did not exist any stable vocalism which could be regarded as belonging to the root but that the vocalism had an apophonic character. However on turning to earlier levels of the language it will be obvious that the personal affixes must have originally existed as separate lexemes and in this case there is no reason to believe that the root morpheme with verbal semantic function would formally differ in any way from that with nominal semantic function; this brings us to the necessity of reconstructing the primary verbal root morpheme as being, in principle, identical in pattern with the primary nominal root morpheme—which means that it must have included a vowel. In the so-called *verba mediae infirmae* we can find even at the historical level certain remnants of the primary root vocalism which, in the different contrasting conjugated forms, did not wholly depend on the laws of apophony.

For proto-Semito-Hamitic these identical patterns of finite verbal forms can really be reconstructed on the basis of historically attested facts, namely:

- \**ja-C<sub>1</sub>əC<sub>2</sub>-*,      \**ja-C<sub>1</sub>C<sub>2</sub>əC<sub>3</sub>-* — perfective, verb of motion or of transitive action;<sup>15</sup>  
 \**ja-C<sub>1</sub>aC<sub>2</sub>-*,      \**ja-C<sub>1</sub>C<sub>2</sub>aC<sub>3</sub>-* — verb of intransitive action;<sup>16</sup>  
 \**ja-CaCaC-* — imperfective, verb of transitive action.<sup>17</sup>

The bisyllabic root pattern \**Sa+CS|CVC*, \**Sa+CS|CV|C+V* is also possible and is actually observed in the case of the so-called 'quadriliteral' verbs.

It would appear that the mechanism of pattern correlation was brought to uniformity; the leading factor must have been the transition from external to internal inflection. The situation in Hausa may serve as a sort of typological model (this does not mean that Hausa RETAINS the original situation in this respect; it is much more probable that a secondary development has here led to results similar to the situation in the proto-language). In Hausa at the end of a finite verb (which here consists of a form of the conjugated auxiliary verb with actor prefix and of a *mašdar*), there is added a vocalic inflection, variously characterizing the verb as stative, transitive etc. Let us assume that in 'pre-proto-Semito-Hamitic' the vocalic suffix -V could similarly be the exponent of intransitive action (-a) or of motion and transitive action (ə-). Then, as a result of a very usual process of absorbing the external inflection into the word-base and turning it into an internal inflection, we would, proceeding from patterns known to us from the nominal root sphere (CS, CVC, CSC, CVCS, CVCVC),<sup>18</sup> encounter such forms as: \**ja-CaS*, \**ja-CəS*; \**ja-CaC*, \**ja-CəC*; \**ja-CaSaC*, \**ja-CaSəC*; \**ja-CaCaS*, \**ja-CaCəS*, with the connotations of intransitive or transitive action respectively. The necessity to distinguish, for verbs of action,

<sup>15</sup> Sem. \**ja-kūn-*, \**ja-qtul-*, *ja-drib-*, *ja-šqī-*; Berb. *i-ssən*, *i-frəs*, *i-gmi* etc.; Cushitic: Beḍauye *e-dir*, *i-fdig*, Somali *ji-qīn*; Chad.: Hausa *ja-n* (an auxiliary verb; the other verbs are composed of a *mašdar* and of the prefix conjugation of the auxiliary verb). Note that while the Semitic languages differentiate the pattern \**ja-C<sub>1</sub>C<sub>2</sub>uC<sub>3</sub>* (the normal type of the transitive verb) and the pattern \**ja-C<sub>1</sub>C<sub>2</sub>iC<sub>3</sub>* (verbs of motion and of transitive surface action), the Cushitic languages have only the latter type which there is the normal one for the transitive verb—probably another evidence of the secondary character of the vowel *u*.

<sup>16</sup> Sem. \**a-hzan-*, \**ja-lbaš-* etc., medial if *a* not in contact with ', *h*, ', *h*. In Berber this type is usually indistinguishable from the preceding, because \**a*, \**i*, \**u* > Berber *ə* or zero.

<sup>17</sup> Semitic Akkad. *i'parras* (apparently with a secondary stress which is responsible for the reduplication of the second radical, since the expected form \**iparas* would, according to rules obtaining in Akkadian, result in \**i'pras*, a form indistinguishable from the preceding pattern). In spite of a wide-spread contrary opinion, I assume that the similar forms of imperfective with full vocalism in Southern Peripheral Semitic (Mehri, Ethiopic, etc.; see Diakonoff 1965, 90 sqq.) are identical with this Akkadian form in their origin; cf. also the Berber imperfective (*ad*) *ifrəs*, (*ad*) *igməḵ* < \**ja-faras*, \**ja-gamaḵ* etc.; Cushitic: Beḍauye *e-ndir*, (*ə*-)*fandig* etc., Somali *ja-qān*; Chad.: Hausa *ja-na*; all these are forms of the imperfective of the transitive (in Hausa: auxiliary) verb.

<sup>18</sup> These patterns should also be regarded as being the origin of the 'participle of state'.

forms expressing momentary action (*punctualis, perfectivus*) from those expressing duration of the action (*durativus, imperfectivus*) would very naturally bring about a system in which contracted (momentary) forms were contrasted with forms with full vocalism (durative):

\**ia*-C<sub>1</sub>aC<sub>2</sub> : \**ia*-C<sub>1</sub>əC<sub>2</sub>,  
 \**ia*-CaS<sub>1</sub>aS<sub>1</sub>, \**ia*-C<sub>1</sub>aC<sub>2</sub>aC<sub>2</sub> : \**ia*-CəS̄-, \**ia*-CəC̄<sub>2</sub>- (or \**ia*-CS<sub>1</sub>əS<sub>1</sub>, \**ia*-C<sub>1</sub>C<sub>2</sub>əC<sub>2</sub>),  
 \**ia*-CaCaS, \**ia*-CaCəS : \**ia*-CCaS, \**ia*-CCəS,  
 \**ia*-CaSaC : \**ia*-CSəC,

which virtually is the verbal system which has been preserved in the historically attested Semito-Hamitic languages.

This situation made it possible to create new verbal roots with forms like \**ia*-C<sub>1</sub>aC<sub>2</sub>a/əC<sub>3</sub>: \**ia*-C<sub>1</sub>C<sub>2</sub>a/əC<sub>3</sub>. In the already mentioned Hausa language we may observe the phenomenon of various consonantal morphs being added to the verb, these morphs gradually becoming part of the lexeme and of the verbal root in question.<sup>19</sup> We may assume that the mechanism of inclusion of the third non-sonorant radical into the proto-Semitic (or proto-Semito-Hamitic) verbal root was somewhat similar. For instance:

\**ia*-C<sub>1</sub>aC<sub>2</sub>+C<sub>3</sub> = \**ia*-C<sub>1</sub>C<sub>2</sub>aC<sub>3</sub>, \**ia*-C<sub>1</sub>aC<sub>2</sub>aC<sub>3</sub>,  
 \**ia*-C<sub>1</sub>əC<sub>2</sub>+C<sub>3</sub> = \**ia*-C<sub>1</sub>C<sub>2</sub>əC<sub>3</sub>, \**ia*-C<sub>1</sub>aC<sub>2</sub>əC<sub>3</sub>, etc.

In cases where no third radical was added, if the original root was biconsonantal the primary pattern \**ia*-CVC might be preserved (sometimes with extension to \**ia*-CVC̄ under the influence of the rhythmical pattern of triconsonantal stems) or there might be created by analogy forms of the pattern \**ia*-C<sub>1</sub>C<sub>2</sub>V : \**ia*-C<sub>1</sub>aC<sub>2</sub>V. Seeing that such forms were impossible with a vocalic suffix, because of the forbidden sequence \*-CVV, a non-syllabic sonant, articulatorily related to the vowel in question (as -' to -a, -i/ʏ to -ə), would be added (of course, if the last phoneme of the root was not already originally a sonant); or, again, the last consonant of the root might be repeated: \**ia*-C<sub>1</sub>C<sub>2</sub>VC<sub>2</sub> (or \**ia*-C<sub>1</sub>VC̄<sub>2</sub>) : \**ia*-C<sub>1</sub>aC<sub>2</sub>VC<sub>2</sub>. It is well known how often the *verba mediae infirmae*, *tertia infirmae*, and *secundae geminatae* alternate in Semitic languages.

Although never quite consistently carried out because of the disappearance of its cause, the differentiation between quasi-normal triconsonantal verbal roots

<sup>19</sup> Another aspect of the same process seems to have been the elimination of such patterns as \**ia*-|CS, \**ia*-C|S-V, \**ia*-|CSC, \**ia*-CS|C-V because they do not allow of the contrasting of momentary and durative forms; once \**ia*-CaS is opposed to \**ia*-CəS (and \**ia*-CaS(a)C to \**ia*-C|SəC) there is no room for a form like \**ia*-C|S-V (or, respectively, \**ia*-|CS|C-V) in the same paradigm.

with the 'weak' consonants  $\text{'}$ ,  $i$ ,  $u$ , as one of their real radicals,<sup>20</sup> and 'weak roots' with an imaginary 'weak' radical<sup>21</sup> (a situation attested in Semitic, in Egyptian, etc.) corresponds in principle to the original differentiation between roots of the pattern  $*C_1\bar{H}C_2$ ,  $C_1u\bar{C}_2$ ,  $C_1VC_2\bar{H}$  etc. and roots of the pattern  $C_1VC_2$ .

The emergence of forms like  $*k\bar{i}s-$ ,  $*m\bar{a}$ ,  $*\bar{i}a-k\bar{u}n-$  which have their origin in rhythmical conditions was, probably, the impetus which in the long run caused the creation of long vowels as separate phonemes. This, along with the differentiation also of  $u$  as a distinct phoneme, greatly extended the possibilities of vocalic inflection, both external and internal. Such extension was most necessary because of the scarcity of other means of word-formation in Semito-Hamitic languages. But the creation of a complicated, ramified and balanced system of verbal stems and nouns of verbal origin (mainly distinguished only by their vocalism from words of related but different meaning derived from the same roots) brought about the functional polarity between consonants and vowels so typical of the Ancient and Middle Stages of Semito-Hamitic. In order not to destroy the associative connections between the manifold nominal and verbal patterns derived from one and the same root, a rigorous conservation of the consonantal skeleton of the root was indispensable. For this reason positional (phonotactic) changes of consonants were at that stage exceedingly rare, the phonological system of Semito-Hamitic consonants remaining astonishingly stable over a period of millennia.

At the same time the secondary origin of the vocalic patterns employing long vowels (e.g. the patterns  $*fa'\bar{u}l-$ ,  $*fa'il-$ ,  $*fa'\bar{a}l-$  as compared with  $*fa'ul-$ ,  $*fa'il-$ ,  $*fa'al-$ ) can still be seen clearly enough and this has repeatedly been noted in Semitological works; the same can be said of the secondary character of  $u$  as compared with  $i$  and of the existence of an original 'bivocalic' stage in proto-Semito-Hamitic. It can also be noted that the spectrum of word-formational patterns in such an archaic Semitic language as Akkadian reveals very few examples of patterns with an  $i$  or  $u$  vowel in the first syllable, while the few such patterns that do exist seem to be of later origin than the others (always excepting the very ancient pattern  $C\bar{a}CS-$  and its imitation  $C_1\bar{a}C_2C_3$ ). It would seem, in general terms, that those patterns of derivative word formation in Semitic should be considered the most ancient which correspond in structure to the primary nominal root morphemes, viz. the pattern  $*fa'l-$ ,  $*fi'l-$ ,  $*fa'al-$ ; already it may be necessary to consider a pattern like  $*fa'il-$  as being the result of the infiltration of an external inflection into the word-base.

The hypothetical character of our results should here once more be stressed; even if they are accepted they will need elaboration, correction and amendment. What seems to me really important is that the structure of the root should be explained proceeding from the actual conditions that existed in the word of which the root was a part. I should also like to remind the reader that the suggested reconstructions

<sup>20</sup> Like Akkad.  $i-s'al$ ,  $i-\bar{s}a'al$ ,  $u-parri'$ , or Hebr.  $\bar{j}i-\bar{s}u\bar{a}h$ .

<sup>21</sup> Like Akkad.  $i-k\bar{u}n$ ,  $i-ku:an$  (or  $i-k\bar{a}n$ ; the symbol  $:$  denoting a glide),  $i-bn\bar{i}$ ,  $i-bann$ , or Hebr.  $\bar{j}a-q\bar{u}m$ ,  $\bar{j}i-bn\bar{a}$ .

refer to a diachronic level different from that to which the laws of apophony studied by Kuryłowicz relate. And it seems to me important to state once again that the prospects of further research in reconstruction of the history and prehistory of the Semitic branch of the Afroasiatic (or Semito-Hamitic) linguistic family seem to me not very promising unless the data of the other branches are considered and the reconstruction of the phenomena in question is attempted at the proto-Semito-Hamitic level—and, in so far as that is possible, achieved. The situation is identical to that of Indo-European linguistics, where the study of one particular branch of the family can lead to only limited results unless the facts of the related branches are brought into the discussion.

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## DISCUSSION

HODGE: I am really rather reluctant to say anything because I can't say it definitely enough. Except that recently I began to wonder whether or not we should take the short vowels of Semitic very seriously at all. There is mentioned in Diakonoff's paper and also in Gazov-Ginzberg's the idea that we may have syllabic consonants, like *ʁ*, *l*, and *r* and so forth—and I wonder again whether or not this is not something borrowed from Indo-European and that Semitic has rather the anaptyctic vowel which we find in say Maghrebi and that most of our short vowels may be due to this anaptyctic occurrence which would be predictable and therefore associated with the *n*, the *l*, the *m* and so forth but would not be *vowel plus m*, and *vowel plus r*, etc., and that what we would then have would be, if you will, long vowels and anaptyctic vowels in proto-Semitic or proto-Afroasiatic.

PARSONS: I feel very bold in venturing into such a deep field as proto-Semitic but it might be worth while for Semitists for me to put one or two suggestions that may have some bearing on proto-Hamito-Semitic. The position of the coda-consonants



in Hausa deserves much deeper study than one has tended to give it. On the phonological level it is perfectly easy to say that this is a CVC syllable and that is of course how one interprets the long vowels of Hausa: by means of an *'alif* which in Hausa you can say is *h*. The only problem is of course how the long *e* and *o* appear in Hausa, but that is a question I do not think we need go into now. There is a curious feature running through Hausa whereby this coda element is very often optional. Nasalization has this kind of sporadic scatter and you get that even in Arabic loan-words: *hakuri* and *hankuri* 'patience' for instance. What the bearing of that on the whole bi-, tri- and quadro-consonantal system of proto-Hamito-Semitic is I just don't know. But there is one very curious morphological fact in Hausa and that is that unlike all the other coda consonants *h* and *n* (but not *m* which is a distinct phoneme although *n* and *m* fall together in many positions) remain invariably as coda consonants. That is to say you cannot have broken plurals in which they initiate another syllable. I'll very rapidly go through class Ic Hausa plurals, which gives you at once I think the picture and you have there of course what Professor Greenberg has written about at some length, the Hamitic—I don't think he said Hamito-Semitic—infixed *-ā-*. For instance if you take *aska* 'razor' you have a plural *asake*, where the *s* coda has become *C<sub>2</sub>*; *turmi* 'mortar', pl. *turame*; *bauna* 'bush-cow', which is clearly from *\*bakna-*, you have with this curious optional feature of labiovelarization (which seems to be incidentally a link between Ethiopic and Hausa) you get *bakane* or *bakwane*; *kaimi* 'spur', pl. *kayame*; *kyaure* 'door', pl. either *kyamare* or *kyaware*, and there is often doubt what the original coda therefore was. But if you get a word like *gwanki*—which is quoted I think in Newman and Ma's list—'roan antelope', it is impossible to have a plural *\*gwanake*, which is what you would expect by analogy. The form just does not occur in any dialect. You get instead *gwankaye* by being treated with what I think is zero reduplication which takes the form of the glide *y*. And exactly the same thing happens where you get a long vowel. So you get *suma* 'hair', pl. *sumaye*; *tsuntsu* 'bird', pl. *tsuntsaye*; and of course geminate *l* develops from *nl*; so you get *kwallo* 'ball', pl. *kwallaye*, and *kyalle* 'piece of cloth', pl. *kyallaye*; and it is a very curious feature, which I don't know whether it has any bearing outside the Hausa or Chadic field, that the pure nasal—homorganic with a following consonant and which one can represent by *N*—is never, so to speak, detached from the syllable in which it appears.

PETRÁČEK: The problem raised by Diakonoff is inspired by an Indo-European problem, namely the role of the sonants within the root. Naturally we welcome the initiative, but we first must define what are the sonants in Semitic. Because what our colleague Diakonoff has employed are the sonants of Indo-European—*r*, *l*, *n*, *m*, etc. And we know now, since Jakobson's analysis in terms of distinctive features, that among the glides are to be included also *'ayn* and *ḥā*. Why are these pharyngeals excluded from the list of sonants? Because, it seems to me, the units are based upon those of another linguistic group, and not the Semito-Hamitic group.



JUNGRAITHMAYR: In continuation of what Mr. Parsons said, especially with regard to the hitherto known Chadic languages, I would like to draw your attention to the fact that there is a great number of trisyllabic roots—which is unusual for Chadic—not only in Hausa but especially in other Chadic languages. These roots have a sonant—to use Diakonoff's term—either *l*, *m*, *n*, or *r*. Now I want to underline the fact that a large number of the roots containing them denote some kind of animal. I would like to draw your attention to examples like the following. (I have pointed out this fact in a paper given last year at the Eighth West African Languages Congress, entitled 'Are *l*, *n*, *m*, *r*, and *k* formative third radicals in Chado-Hamitic animal names?'). You have in Hausa *ḡauna* but all the cognates in Chadic languages for 'buffalo' miss the *ba*- prefix, as I would say, and you have forms like *kāḡen* in Tangale, *kāḡan* in Sura, and so on. I think we would be able to list at least about twenty Chadic languages in which you have this triradical form for 'buffalo'. The same applies to 'crocodile', which is reconstructed by Newman-Ma as *\*k-r-m*; you have Hausa *kada* but undoubtedly with a lost third radical, in comparison with the other Chadic languages. And I should like to remind you, with reference to what Mr. Parsons said, that we have in the plural of *kare* 'dog' in Hausa, *karnuka*, a form with a third radical, whether original or of some other origin we cannot say.

PARSONS: *Karne* has recently been attested as occurring in rustic Kano.

JUNGRAITHMYAR: I want to refer to other examples like 'ostrich' which is widely spread in the Chadic area: *t-l-m*, or 'ram' reconstructed as *\*n-g-m*, and so on. I don't want to go into details here but I have collected about twenty-five animal names, which can be found to be attested in a large number of Chadic languages and which have, in comparison with other roots in Chadic languages, a third radical which does not seem to be a part of the root etymologically.

BARR: Would no Assyriologist like to comment on the question about the forms inflected with *u* being both locative and nominative?

VYČIHL: I said it about ten years ago in a paper published in the *Rivista degli Studi Orientali* on the Arabic declension, where I pointed out the identity of both cases.



## CONTRASTING ARTICULATIONS IN THE MODERN SOUTH ARABIAN LANGUAGES

T. M. JOHNSTONE

Since ejectives occur in the languages of the Cushitic group (Bedawye, Agaw, Saho-  
'Afar, Sidamo, Galla, and Somali),<sup>1</sup> but have not so far been recorded for the Semitic  
languages outside the Ethiopian area, it might reasonably be concluded that this  
was a N.E. African rather than a Semitic phenomenon.<sup>2</sup>

It is of interest for this argument that recent field work carried out by the writer  
in Socotra and Oman shows that ejectives occur in the Modern South Arabian  
languages, namely Mehri, Ḥarsūsi, Šheri and Socotri. Experimental work done with  
Mr. J. Carnochan of the Department of Phonetics and Linguistics of S.O.A.S.,  
with the help of a native speaker, has confirmed the position in regard to Mehri.

The series of contrasting articulations which occur in these languages is:

<i>t</i> ( <i>tʰ</i> )	<i>t</i> '
<i>k</i> ( <i>kʰ</i> )	<i>k</i> '
<i>s</i>	<i>s</i> '

In Mehri, Ḥarsūsi and Šheri there occurs also the contrasting triad

ʃ	š	ʃ'
---	---	----

(in which *š* represents a voiceless lateral and *ʃ'* an allophone of *k*', occurring most  
frequently in the contiguity of high front vowels) and the contrasting triad:

ð	ž	ð
---	---	---

(in which *ž* is a voiced lateral with phonemic status, and in Šheri (only) also an  
allophone of *l* occurring in the contiguity of high front vowels).

<sup>1</sup> On the distribution of the ejectives in the Cushitic languages cf. Tucker and Bryan (1966), 498.

<sup>2</sup> Some Semitists have, however, claimed that this was a proto-Semitic phenomenon. Cf. e.g. Ullendorff (1955), 155. Ullendorff quotes Brockelmann (1908-1913), I 121, and a much more categorical statement by Bergsträsser (1928), 5.

Unlike *t'*, *k'*, *s'* and *tʃ'*, the consonant *δ*, which occurs relatively infrequently, is not an ejective, though on historical grounds it is to be classed with them.<sup>3</sup> *δ* is a voiced or partially voiced consonant in Mehri, Ḥarsūsi and Šheri, and experimental work on the mode of articulation of this consonant indicates that in the case of *δ* there is a glottal closure before the onset of voice.

It is interesting at this point to note that *s'* in all the MSA languages has a voiced or partially voiced variant, occurring mainly in intervocalic positions, which native speakers sometimes confuse with *z*. In fact, however, considerably more muscular tension is involved in the articulation of the (partially) voiced variant of *s'* than in the articulation of *z*. It seems likely that this variant is to be classed with *δ* as pre-glottalised.

A tape was played at the Colloquium giving some examples of the occurrence of these consonants in MSA, and some contrastive examples from Mehri. These examples are set out below.

## (a)

Ḥ	M	Š	
<i>t'aad</i>	<i>t'aat'</i>	<i>t'ad</i>	'one'
<i>šiwit'</i>	<i>šiwuut'</i>	<i>šot'</i>	'fire'
<i>k'āduur</i>	<i>k'āduur</i>	<i>ek'aθ'ar</i>	'he was able: the leopard'
<i>ḥafərook'</i>	<i>ḥafərook'</i>	<i>dək'iik'</i>	'settlements: sheep-pens: flour'
<i>yəs'aws'</i>	<i>δ-yəs'aws'</i>	<i>es'ɔd</i>	'he fears: the fish'
<i>ḍaar</i>	<i>ḍaar</i>	<i>(δir,θ'ir)</i>	'on'

## (b)

<i>toob</i>	<i>t'oob (t'ɔɔb)</i>
'he repented'	'kind of tree'
<i>kal</i>	<i>k'al (k'æl)</i>
'all'	'he poured'
<i>soobər</i>	<i>s'oobər</i>
'always'	'patient'
<i>ḍə-luum</i>	<i>ḍəluum</i>
'he expected, anticipated'	'he wronged'
<i>səetəl</i>	<i>səet'əl</i>
'side'	'bucket'
<i>ruuka</i>	<i>ruuk'a (ruuk'æ)</i>
'he hopped'	'patches'

<sup>3</sup> In Šheri *δ* is freely variant with an ejective *θ'*.

<i>ð-isaliən</i>		<i>ð-ies'alīən</i>
'he amuses'		'he prays'
<i>ħəðawr</i>	<i>ħəžawr</i>	
'they made a sheep pen'	'he was present'	
<i>fəruut</i>		<i>fəruut'</i>
'he changed his words'		'it went without (me)'
<i>səbuuk</i>		<i>səbuuk'</i>
'he put together'		'he came fast'
<i>lās</i>		<i>lās'</i>
'he wormed his way in, flattered'		'he moved close to s.o.'
	<i>məreež</i>	<i>məreeð</i>
	'illness'	'instruct!'

Speakers commenting on the taped examples compared the ejectives in MSA with their equivalents in the Ethiopian languages and with Hausa. The fact that a prosodic or concordial relationship exists as between vowels and consonants was noted by a number of speakers. The phonetic transcription, added in square brackets in some cases, gives some idea of the nature of this concordial relationship. In general the quality of the vowels in such prosodies is opener than is otherwise the case.

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#### DISCUSSION

JOHNSTONE: I might just say that we have made some physical measurements of these sounds, and my colleague Jack Carnochan has said that he will discuss this matter with you if you should wish to ask him any questions about it.

LESLAU: I do not need to say that this is a minor revolution in so far as Semitic is concerned. We always thought glottalization was only a privilege of Ethiopic and some of the Cushitic languages. We have known Mehri, Šheri, Socotri for quite a while but we were never told that they had glottalized consonants. Of course it is

of extreme importance what we have in front of us, but the question still remains: are these proto-Semitic features, or is Arabic still the language that is closest to the proto-Semitic type? We thought that the Arabic type, that is with velarization, was proto-Semitic and that Ethiopic with its glottalized consonants had been influenced by Cushitic. But possibly we may have to change our opinion. On listening to the recording however I had the impression that the glottalization did not sound to me to be of the same type as that in Ethiopic. Another problem is that of the influence of these glottalized consonants on the vocalic system. Now in Ethiopic they do not influence the vowels, the Arabic emphatics do. In Ethiopic they have no influence at all, but apparently in modern South Arabic there is some influence of one type or another, and all this would be an extremely interesting subject for further investigation.

PALMER: I am sure that Professor Leslau will allow me to correct him on one point with regard to his last remark. This is not true of Tigre, where there is a difference between the quality of especially the open vowels with ejectives and the non-ejectives.

NATALIE WATERSOHN: Speaking entirely as a phonetician I would say I was struck by the similarity of the glottalization of the consonants with those of Amharic.

TUCKER: On listening to this tape one thing did strike me and this was that *ruuka* definitely ended in a glottal stop, and I noticed that one or two words which ended in an *l* or *n* had a glottal stop after them. Now this phenomenon occurs in Somali where one ought to distinguish between consonants which end in a sort of *h* and consonants which end in a glottal stop. So that is another tie-up with at least one Hamitic language.

SKINNER: It seems rather bold just to pick on one resemblance to Hausa but as it does happen to have a glottalized *s* I think it is worth mentioning. The Hausa word for 'fear' has I would say a root GLOTTAL 's, w and r—'swr—and might be compared to the fifth example, *yəs'aws'*.

CARNOCHAN: I do not particularly want to go into the phonetic details of these articulations but I should have thought that many of you would have agreed that the ejective quality was very much more noticeable in some words than it was in others, possibly the *k* ending words and so on, the resonance of these was perhaps more clearly audible. With regard to Professor Tucker's point about the final glottal stop, if we look at Fula, all consonant final nouns do in fact end with a glottal stop after the consonant.

PARSONS: The feature of final glottality links with the feature of femininity in Hausa (a) in numerals, (b) in adverbial nouns, (c) in proper names, (d) in names of cities, countries and rivers.

PETRÁČEK: This article poses extremely interesting problems along the lines indicated by Professor Leslau.

KAYE: As regards whether glottalized consonants are proto-Semitic or not, I think a paper by Knudtzon about fifteen years ago proved very convincingly that Akkadian for example had glottalized consonants and that they are the primary type.

HODGE: We have not mentioned any possible historical factors here, but it would seem to me very possible that the glottalization was something which was added to the syllable in the proto-language, because it does move from consonant to consonant and it is prosodic in several of the Hamito-Semitic groups. I think we ought to think of this as something which was possibly in the proto-language a separate morpheme.





## LE DYNAMISME DU SYSTÈME PHONOLOGIQUE PROTOSÉMITIQUE ET LES PROBLÈMES DE LA PHONOLOGIE CHAMITO-SÉMITIQUE

KAREL PETRÁČEK

1.0. Le rapport que je viens de présenter à ce public honorable s'efforce de répondre à la deuxième question du programme de notre Colloque : *What can be reconstructed of the phonology, grammar and lexicon of the proto-language of the group?*

Voici le premier problème qui se dresse devant nous : qu'est-ce que signifie 'TO RECONSTRUCT' — reconstruire? Il ne s'agit pas — sans doute — de former une réalité qui avait fonctionné dans une société préhistorique, mais plutôt de constituer UN CONSTRUIT (*construct*) LINGUISTIQUE, donc une entité dont le champ d'application est défini par la linguistique, par LA SCIENCE linguistique. Le concept de 'reconstruire' — ou plutôt CONSTRUIRE — appartenant à la zone d'une métalangue (linguistique), sera alors limité par la syntaxe de cette métalangue, c'est-à-dire par le niveau de la linguistique respective : dans notre cas de la linguistique nommée sémitique et d'une autre linguistique que nous voulons appeler Ch-linguistique craignant (vous savez pourquoi) d'employer l'appellation 'chamitique'.

Cette dernière linguistique trouve son objet dans les cinq branches de la famille chamito-sémitique. Quelques-unes posent en outre des problèmes graves (la branche couchitique par exemple).

La syntaxe de ces deux linguistiques mentionnées est assez différente : dans la linguistique sémitique on travaille avec un protosystème assez développé ce qui n'est pas en ce moment le cas dans les quatre branches de la Ch-linguistique (voir p. ex. le tchado-hamitique; les obstacles se montrent dans le couchitique; l'égyptien est employé comme une langue concrète, etc.). Il est alors assez difficile de comparer un CONSTRUIT LINGUISTIQUE (un protosystème) avec un système linguistique actuel (une langue concrète). Nous savons comment M. Diakonoff voulait éviter cet obstacle en parlant de trois étapes (*stages*) historiques (ancien, moyen et nouveau) et en s'efforçant d'employer les formes reconstruites. Mais le problème posé par nous reste : on désigne p. ex. le beɗaɗye et le somali comme le couchitique nouveau (v. dans la phonologie, T. I) mais on vient de comparer un construit linguistique (le berbéro-libyen, nouveau) avec une langue concrète (un système actuel).

Sans doute, notre modèle de la langue concrète (p. ex. égyptien) représente aussi un construit linguistique, mais un construit d'un autre grade et d'une perspective

plutôt synchronique que diachronique. Le protosystème entre sans doute dans la diachronie.

Il ne reste alors — pour être fidèle à une méthodologie pure — que de reconstruire (ou mieux de construire) les protosystèmes ou les segments de ces protosystèmes que nous voulons employer. C'est une constatation assez banale. Mais en tout cas nous avons touché deux concepts contenus dans la question posée pour le Colloque : le concept de 'reconstruire' et celui de la 'protolangue' qui avait été transformée par nous en concept d'un protosystème ce qui n'était pas par faute ou par hasard. La conception systémologique — si vous voulez structuraliste — manifestée dans la dénomination 'protosystème' avec toutes les conséquences de ce mot bien défini aujourd'hui, vient d'apparaître dans le troisième terme employé dans la question No. 2 pour le Colloque, dans le terme de '*phonology*'. En anglais on a l'habitude d'appeler ainsi ce que nous pouvons nommer dans d'autres langues 'la phonétique'. Sous le terme *phonology* (en fr. 'phonologie') nous voulons comprendre la phonétique fonctionnelle (al. *Phonologie*, fr. 'phonologie'), c'est-à-dire ce que les Anglais et les Américains désignent par le terme '*phonemics*'. Mais il ne s'agit pas — au moins chez moi — seulement des mots, de la terminologie : le contenu en est absolument différent. La phonologie départ de la pensée systémologique et s'occupe des systèmes, ce qui n'est pas le cas pour la phonétique.

Poussons alors notre pensée plus loin dans le sens indiqué : ce qui nous intéresse dans la construction (et puis dans la comparaison) d'un protosystème ce sont les traits d'un système phonologique et non seulement les matériaux phonétiques.

Le dernier concept qui exige un mot d'explication est le concept d'un groupe de langues (*the group*). En général, c'est un problème assez grave — voir les définitions possibles en termes généalogiques, typologiques ou en termes de la linguistique aréale (à Prague on a parlé des 'unions des langues' — *Sprachbünde*); en particulier — dans le cas des langues sémitiques — on peut intuitivement accepter le groupe tel que nous le connaissons (c'est-à-dire dans le sens génétique). Mais cette opération qui facilite nos réflexions signifie en même temps l'acceptation du critère généalogique qui est au fond du concept de groupe de langues sémitiques. En abordant de nouveau le problème sous son aspect général, il nous faut poser la question si ce concept d'un groupe de langues imbu par l'historisme et par les relations du type généalogique serait valable aussi pour d'autres branches du chamito-sémitique et pour le chamito-sémitique (c'est-à-dire pour le protosystème supposé). Ce n'est pas certain : on a assez de possibilité de réfléchir sur le problème de différentes branches du chamito-sémitique en termes non-génétiques. Voici un problème pesant sur nos travaux qui s'efforcent d'unir les données linguistiques avec celles de l'anthropologie, de l'archéologie et de l'histoire de la culture.

Mais heureusement je peux et je dois rester dans le cadre particulier des langues sémitiques en évitant ainsi la solution de ces problèmes.

2.0. Forcé par le programme et par mes réflexions d'ordre méthodologique je vais

me borner au système phonologique proto-sémitique ou plus précisément à son segment en arrière que j'accepte comme caractéristique pour le protosystème en question. Je m'intéresse alors aux séries de localisation en arrière : glottale, pharyngale, postvélaire et à la corrélation nommée habituellement emphatique-pharyngalisée (ou vélarisée). D'après ma conception, cette corrélation est traitée comme appartenant à la zone d'articulation en arrière par son trait pharyngalisé (glottalisé dans les langues éthiopiennes) qui forme le component de cette corrélation 'd'articulation accessoire' (le trait accessoire). Pour avoir la possibilité de résoudre quelques problèmes il me fallait concevoir cette corrélation d'articulation accessoire (chez Trubetzkoy, *Nebenarbeitsreihe*) purement en termes phonologiques et plus largement que ne le fait la sémitologie qui départ plutôt des données phonétiques. Mais ce sont des problèmes qui dépassent notre sujet.

2.1. Le système protosémitique est assez bien défini grâce aux travaux de différents sémitisants (il faut souligner J. Cantineau) et est entré dans la dernière synthèse de la plume de S. Moscati (avec d'autres), déjà en quelques égards en termes phonologiques.

Dans la zone d'arrière le système est dressé comme il suit :

$$\begin{array}{cccc} k & g & q & ch \ gh \\ & & h & ' \\ & & ' & h \end{array}$$

(j'ai changé la suite *k q g* fautive en *k g q* et j'ai posé la hamza sous *q* comme l'est chez Moscati).

Ajouter la corrélation 'd'emphase'

$$\text{ʔ} \quad \text{ʔ} \quad \text{q̣} \quad (\text{latéralisé}) \quad \text{s}.$$

Sans discuter la position de différents phonèmes dans le système, il nous faut signaler que ce segment du système dressé pour le proto-sémitique ne reste pas sans difficultés d'ordre heuristique et méthodologique.

Les séries glottale et pharyngale peuvent rester pour le temps sans discussion.

Mais ce qu'il faut discuter c'est la tension interne dans la série pharyngale qui a abouti finalement soit à la disparition de celles-ci, soit à leur transposition dans l'espace buccale formant ainsi le premier élément de la série postvélaire (*ch*) qui par le développement systémologique a conduit à la formation de la série postvélaire *ch-gh*. Ainsi le phonème ghain ne peut rester dans le protosystème (sans discuter pour ce moment le problème du *ch* qui est dans notre perspective phonologique du même ordre que *gh* mais situé différemment dans le temps). Vous connaissez nos travaux qui tâchent de prouver notre conception qui nie l'appartenance du ghain (en qualité d'un phonème!) au système du protosémitique. Ce que je veux souligner

ici c'est le dynamisme interne de la série pharyngale en marquant deux étapes de son évolution ( $h$ -',  $h$ -'  $ch$ - $gh$ ).

Un dynamisme analogue peut être observé dans la corrélation dite emphatique-pharyngalisée (qui contient les phonèmes pharyngalisés et un phonème latéralisé). L'interdentale emphatique latéralisée  $\underset{h}{q}$  (ou  $\underset{h}{q}$ ) tient le status phonologique seulement dans les langues sémitiques du sud et il ne serait possible de le prouver dans le proto-système que par hypothèse (voir l'expression prudente de S. Moscati : « phonological correspondences would nevertheless suggest its existence »). On suppose alors la déphonologisation d'un phonème de cette corrélation et l'arabe et les autres langues sudsémitiques doivent prouver le fait. Mais le développement des langues sudsémitiques et surtout (mais non seulement) de l'arabe justifie une tendance tout à fait différente : l'accroissement de la corrélation d'emphase (pharyngalisée). Nous le connaissons bien dans le cas de  $l$  (traité par moi et par Ch. Ferguson) ainsi que dans différents dialectes modernes où le status phonologique des autres phonèmes emphatiques-pharyngalisés est absolument affirmé. Cette tendance d'élargir la corrélation mentionnée pose devant nous la question si dans le cas arabe (et sud-arabique ancien) le système graphique n'avait pas enregistré un état TRANSITOIRE de l'évolution de cette corrélation dans le sens mentionné, c'est-à-dire de l'état  $x$ - $\underset{h}{q}$  en train de développement vers l'état encore plus large :  $x$ - $\underset{h}{q}$ ...  $l$ ...  $r$  etc. Dans ce cas il serait inutile d'observer l'arabe comme la langue la plus conservatrice — au contraire! Voilà le dynamisme de la corrélation en question qui va se joindre au dynamisme cité plus haut pour les pharyngales étant en train de se développer en postvélares (surtout pour le ghain) et qui rend assez suspecte la position au moins de deux phonèmes (ghain,  $\underset{h}{q}$ ) dans le protosystème. Si ces deux phonèmes ont manqué dans le système, sa structure interne diffère de celle qui a ces deux éléments. Nous avons ainsi attaqué le système en entier (vu sa section en arrière).

Ajoutons encore que même le phonème  $q$  et sa position dans le système (on a la tendance de le classer parmi les emphatiques vélarisées ou pharyngalisées) n'est pas sans problèmes étant donné la non-occurrence d'un travail accessoire pendant son articulation. Il peut représenter au contraire le deuxième élément de l'opposition de  $k$  en avant contre  $k$  en arrière ( $q$ ). Mais ce problème reste encore à résoudre.

Ajoutons finalement comme un fait analogue le développement du dernier membre de la section en arrière et son développement spontané  $g \rightarrow \check{g}$  (palatalisé). Le caractère secondaire de ce développement est prouvé par des données d'ordre comparatif et est reconnu par la sémitologie. Mais les autres cas mentionnées par nous ne représentent-ils pas la même situation? Je le crois.

Il faut accepter le dynamisme du système phonologique d'une langue (ou d'un groupe de langues) dans toute son ampleur et ne pas projeter le résultat de ce dynamisme dans le point de départ, dans un protosystème et c'est justement ce que font plusieurs des sémitisants.

3.0. On le fait non seulement pendant la reconstruction du système protosémitique,

mais aussi en reconstruisant le protosystème chamito-sémitique ou le ghain et une ample corrélation d'emphase pharyngalisée (du type arabe) ne cessent d'apparaître (quelquefois avec un point d'interrogation).

Il serait assez facile de prouver la possibilité d'accepter le dynamisme mentionné dans la construction du système protosémitique par des données de différents groupes du chamito-sémitique, ce qui n'est pas notre devoir aujourd'hui. Disons seulement que la postvélaire ghain ne trouve pas son contre-part (sauf les cas aussi secondaires en berbère) dans les autres groupes du chamito-sémitique. En ce qui concerne la corrélation de travail accessoire (emphatique pharyngalisée en sémitique), le problème est compliqué au point de vue d'articulation, p. ex. dans les langues couchitiques, et il me semble utile de parler plutôt de différentes corrélations partielles ou séries d'articulation. N'oublions pas que quelques langues archaïques ne connaissent pas les emphatiques (Agau sud d'après J. Tubiana et F. R. Palmer). L'égyptien prouve aussi notre thèse (pour le ghain et pour les emphatiques pharyngalisées du type sémitique — le problème du *d* égyptien est à résoudre aussi en comparaison avec les langues couchitiques et non seulement sémitiques). Pour le berbère nous pouvons accepter seulement une corrélation d'articulation accessoire restreinte. En tchado-hamitique (en tant que nous sommes capables de le voir en entier et non seulement haussa) la corrélation d'articulation accessoire est d'un autre type que celle-là en sémitique.

4.0. Après avoir discuté une section du protosystème supposé du sémitique nous pouvons répondre à la question posée dans notre Colloque assez brièvement : vu le dynamisme des systèmes concrets sémitiques on est amené à ne pas négliger la même possibilité dans le protosystème sémitique qui doit — en tout cas — être construit d'un inventaire des éléments plus restreint que ce ne soit d'habitude dans les études courantes et dont la structure serait — dans ce cas — différente des structures des systèmes concrets. Cette hypothèse acceptée, on en peut déduire une conséquence pour la construction du protosystème chamito-sémitique, si nous sommes déjà un jour capables de le faire d'une manière qui serait en conformité avec nos prétentions méthodologiques : Nous pourrions utiliser des systèmes des langues sémitiques historiques et du protosystème de ces langues MOINS que nous ne le faisons encore aujourd'hui. Ce MOINS conçu ici matériellement et quantitativement (l'inventaire des éléments en question et leur qualité phonétique et phonologique) représente d'autre part un PLUS méthodologique.

#### DISCUSSION

PETRÁČEK: The problems I have dealt with are both of a methodological and of a practical order. Taking the question "What can be reconstructed of the phonology, grammar and lexicon of the proto-language of the group?" I would myself ask

“What do we mean by RECONSTRUCT?”. We spoke of this question, whether to reconstruct or not, yesterday. Well, I myself would prefer to say ‘construct’ because, in fact, we reconstruct nothing. We construct, so to speak, a logical model which operates in the diachronic plane but it is only a model, it is not the reality. On the question of comparing proto-forms or present day forms, one can do either but what one must not do is to compare proto-forms, that is to say those of the model, with modern forms—something that is, however, often done. Regarding phonology, I would underline the **SYSTEMATIC** nature of phonemic systems. On this basis I have recently analyzed two areas of the Semitic or proto-Semitic phonological system, namely the back series—the laryngals and post-velars, and the velarized or pharyngalized emphatics. In both these areas I have been able to demonstrate—it is the problem of the *ɣayn*, a shadow on my existence!—that the pharyngals have a certain tendency to evolve towards the post-velars, and that the emphatic series has a tendency towards expansion. We know the situation from Arabic—the emphatic *l* which is first a mere phonetic variant then in modern Arabic a phoneme—it is probative. But what do people do? They take the results of those two tendencies and set them up as if that were the situation at the beginning of the evolution—which I cannot accept, neither where the *ɣayn* is concerned nor the whole emphatic series. That is why at the end of my article I say that we can make less use of the systems of the historical Semitic languages or of the proto-system of these languages than we do today.

LESLAU: Do you then consider that the *ɣayn* in South Arabic arose because of phonetic considerations?

PETRÁČEK: Also.

LESLAU: But not specifically because of phonetic considerations?

PETRÁČEK: Also under pressure of *r*. For example in South Arabian, under pressure of phonetic elements.

LESLAU: Would you then consider—as, if I am not mistaken, Růžička thought—that the *ɣayn* in Arabic would be mainly phonetically conditioned?—because of certain consonants, certainly the *r*, and so on. Is this the case?

PETRÁČEK: Well, it is primarily, as I have said, not a phonetic problem but a phonemic one. Because this passage of *ʿayn* to *ɣayn* took place in two stages: firstly phonetic, perhaps under influence of the environment, then a second stage in which *ɣayn* became established as an independent phoneme. Whereas in, for example, Ugaritic we can observe this development towards the status of phoneme, in South Arabian and North Arabic we already have it as a full phoneme. The tracing of

the development is therefore difficult. I have recently analyzed the facts for Ugaritic and, if I am not mistaken, more than fifty per cent of the *ḡayns* in Ugaritic are in the neighbourhood of a liquid, *r* or *l*, in the root—which proves something. Naturally the development is a continuing one; it has already gone much further in Arabic so that we have quite a different system. So what I wanted to say was that in the proto-system there was no *ḡayn*—absolutely none.

ULLENDORFF: If I understand Professor Petráček correctly his reconstructions develop beyond those of Růžička—there is a further development now. Now, you say there was certainly no *ḡayn* in proto-Semitic, but how do you then explain the occurrence of *ḡayn* in such disparate languages, which you mentioned yourself a moment ago, as Ugaritic, South Arabian and Arabic?

PETRÁČEK: I was speaking of Arabic as a whole. It is a problem which concerns only Arabic—roughly speaking North Arabic and South Arabian and Modern Arabic. Regarding the analysis of the roots of modern Arabic and South Arabian, I have published an article in the *Festschrift Brockelmann*. There is quite strong evidence for the development. If you compare Mehri, Šaḥri and others you find many arguments in favour. So what I wanted to say was that the *ḡayn* developed as an independent phoneme in South Semitic—Arabic and South Arabian and Modern Arabic. The proto-Semitic question is quite another matter.

ULLENDORFF: And Ugaritic?

PETRÁČEK: As I said, it is the beginning of the development. It is the tendency in the Semitic languages, pharyngals towards postvelars. Sometimes the pharyngals have remained or they have disappeared.

ULLENDORFF: Accepting this point for argument's sake, in what way would the development of *ḡayn* then differ from any other consonants in Semitic which you presumably would accept as proto-Semitic?

PETRÁČEK: I have only studied the back series of the proto-Semitic system, that is to say the pharyngals and post-velars. So the others I cannot argue about, but they ought to be studied. What I would like to add is that there is also strong supporting evidence in the 'Hamitic' languages. Where is the *ḡayn* in Egyptian? In Berber it is secondary, as Vycichl—or was it Zylharz?—has pointed out. So there is very good evidence on this problem of the *ḡayn*, and of the emphatics although that is another matter.

LESLAU: Just one question. You seem to want to make a generalization for Semitic, namely pharyngal going toward a post-velar. But you have given only one case of this, the *ḡayn*. Do you have other examples of this general principle?



PETRÁČEK: Well, what I was able to show was that there was this development towards the post-velars. And because it was limited to Arabic, that is to say to South Semitic, I can only speak for South Semitic. What we have then is a positive argument, so to speak, whereas for the other groups there are only negative arguments, namely that the development is not the one that we know for Arabic.

VYČIHL: What corresponds to *ɣayn* in Egyptian is *ḥā'*. When there is a voiceless consonant we have in its proximity a *ḥā'*. For example, corresponding to the Arabic root *ws'* 'to be wide' we have *wšḥ*, for the number 'seven' (*sab'a* in Arabic) we have *sfḥ*. And this demonstrates that the situation was not the same as in the Semitic languages. And there is another case which is disturbing, we often find in Egyptian an *'ayn* and a *ḥā'* in proximity. But it is unthinkable in a Semitic language that there should be a *ɣayn* plus a *ḥā'*, that one should be pronounced next to the other. We can prove it—for example we have a word for 'moon', *y'h*, but no one can pronounce it any more than one can pronounce a *z* next to an *s*. It is impossible. We have another case, *s'h* 'to set upright'. So there is a mystery here which has not been solved, which shows that Professor Petráček's research is of great value and should be pursued.

J. BYNON: With regard to this problem, in Berber the *ɣayn* is in a rather peculiar position, it is isolated. For, as we know, the *qāf* in Berber was originally simply the geminate form of the *ɣayn* and it is only due to the influx of loan-words from Arabic that it may now have acquired the status of independent phoneme in certain Berber dialects. And the same thing can be said of the *ḥā'*, which appears originally to have simply been the allophone of *ɣayn* in voiceless clusters, etc. This would leave it isolated in the system. Now, if we look at loan-words from Latin—words like *taɣawsa* 'thing' which comes from *causa*, *tabya* 'rosehip, blackberry, etc.' which appears to come from *baca* 'berry', *ɣušt* 'August' from *Augustus*—a *ɣayn* in modern Berber frequently corresponds to a */k/* or a */g/* in Latin. We have also the examples of the first person singular personal affix of the verb, which is a *ɣayn* or a *ḥā'* in most Berber dialects but a *kāf* in Zenaga, and of the free form of the personal pronoun *nakk*, which would seem to correspond to Akkadian *anāku*, Ancient Egyptian *inḳ*, etc., faced with the bound forms *-anɣ*, *-aɣ*, etc. All this does make one wonder whether the *ɣayn* is really an original phoneme in Berber, especially in view of its isolated position in the system. I do not know whether this would account for all *ɣayns*, but it certainly deals with quite a lot of them, and I think we should keep an open mind on this question. Especially as there are certain other Berber words, like *nəɣ* 'to kill'—if you imagine that the *ɣayn* perhaps corresponds to a */k/—ixf* 'head', *xəf* 'upon, etc.', which remind of Latin *neco*, *caput*, etc., although the connection is not necessarily with Latin (there are similar forms with */k/* or */g/* in Dinka, Luo, Nuer, Shilluk, etc.)—*taɣaɬt* 'goat', *lləɣ* 'to lick'. There are quite a number of words like this in which a *ɣayn* seems to correspond to a */k/* or a */g/*. They may be loans, although they are not normally thought of as such. At any rate, I do not think that we should consider *ɣayn* as necessarily original in Berber.



### **III**

#### **ANCIENT EGYPTIAN SECTION**



## EGYPTIAN AND SURVIVAL

CARLETON T. HODGE

For some one hundred years the major evidence quoted for maintaining that there was a group of related languages including Semitic and Egyptian was morphologic (e.g. Benfey 1844; Barton 1934, 17-25, tables; Lefebvre 1955, 1-4). Calice (1936) followed by Cohen (1947) were major works aimed at the establishment of related vocabulary and sound correspondences. These are illustrative of the two major trends in the study of AAs (HS): comparative morphology and comparative phonology.

While many scholars have assumed the usefulness of both (as, e.g., Cohen 1947) others have felt that morphology was somehow more reliable. It is not that there was objection to comparative phonology as such, that is, to the setting up of sound correspondences. There was rather the feeling that the data were insufficient to establish such rules and that the preliminary stage, the comparison of vocabulary, was too unreliable. Polotsky (1964, 360, n. 10) and Rundgren (1961, 110) have expressly stated their preference for morphological evidence in determining linguistic relationship. Ullendorff has stressed the drawbacks of vocabulary comparison (1958, 72; 1961, 28). Tucker has several times advocated the use of grammatical data, citing the woeful lack of lexical material (1957; 1967).

At the other extreme, Greenberg has used vocabulary comparison as a classificatory device without establishing phonologic correspondences. While others have felt this to be a preliminary stage in the process, Greenberg has held that the results are much more definite. It is therefore necessary to list lexical comparison as a primary tool currently being used, along with morphology and phonology. This is not to judge its effectiveness but to make a clear differentiation between it and comparative phonology.

Closely associated with both morphologic and vocabulary comparisons but not as yet exploited in any systematic way is comparative semantics. Pursuit of current thought concerning the latter is illustrative of what one might term academic drift, to paraphrase Sapir. In 1940 we find Whorf making the very suggestive statement, in reference to certain semantic features, "One concludes that such categories are but linguistic kinship systems, and like social kinship systems do not follow any

universal norm" (1956, 161). This approach ("the structural and semantic patterns") has been advocated by Ullendorff as offering "yet another means of determining what a Semitic language is" (1958, 75; see also 1961, 28-29).

There has been increasing use of, and discussion of, semantics in descriptive linguistics (see, e.g., Ullmann 1966; Weinreich 1966 and references). The most common technique adopted has been the formulation of semantic 'features', which are intended to be universals. While such study is still very much in the formative stage, it is essential that semantic universals be discovered and stated in order for Whorf's suggestion to become a useful tool. Weinreich, a major contributor to the semantic field, poses the problem thus: "Despite the basically arbitrary quality of semantic 'mapping' displayed by languages, there are nevertheless remarkable parallelisms between both related and unrelated languages. How are these parallelisms to be formulated and quantified?" (1966, 143).

The implications are clear. Semantics, it is assumed, will follow the same patterns as other aspects of language. Likenesses will be classifiable as "*generic* (inherent in all units within the frame of analysis) or *convergent* (due to chance ...) ... *genetic* (... from a common ancestor) or *diffusional* (transmitted ... subsequent to the period of any common ancestor)" (Hymes 1964, 567). The semantics of the languages of the world must be examined, those motifs found to be universal ('generic') abstracted and the others tabulated and studied. When such a project will be begun, let alone finished, is a completely unknown factor. In the meantime the scholar will continue to speculate on both the universal and non-universal motifs. It would appear probable that the universals will be or include as a core those based directly upon perception.

Another aspect of the problem is the fact that the relationship of what may be considered as primary motifs to linguistic forms is by no means a one-to-one ratio. On the contrary, it is generally assumed that these motifs bundle as 'features' in relationship to a given linguistic form. A corollary of this assumption would be that even given universal motifs, their bundling as features is demonstrably not universal in a great many of cases. Indeed, there may not be any bundling which is universal. This does not necessarily mean that a universal bundle, if discovered, would then be treated as a single motif, as the existence everywhere of the association of motif A and motif D, AD, does not preclude the independence of A and D, the occurrence of AB, AC, etc.

Comparative syntax is another relatively unused tool. At present it is employed only with languages known to be related (e.g. Indo-European), not as a classificatory device, as the general distribution of syntactic constructions must first be established.

The five possible approaches to our problems are, then, comparative phonology, morphology, syntax, lexicon, and semantics. It is, I believe, agreed by all that a comparative study with well-established phonological correspondences is the desideratum. The strongest advocate of the morphologic approach would welcome a clear and well-founded statement of sound correspondences. Those who are sceptical question its feasibility, not its desirability (as noted above).

Given the situation as it is, with a very unsatisfactory phonological picture, what hope is there of success in the near future? Or can we hope for success at all? We have before us the example of such perceptive linguists as Sapir, who was able to set up groups of languages as related long before such relationship could be demonstrated by such reliable data as sound correspondence. If we assume, as I believe we must, that Sapir's success was due not to some undefinable intuition but to utilization of some of our five categories of comparison, can we state these categories in such a way that they become tools usable by anyone? Have they been used, unwittingly, by earlier students of AAs (HS)? What is it about the grammatical approach which gives its proponents so much confidence? Are they, in fact, dealing with bundles of semantic motifs, coupled with formal similarities?

Within the framework of the above five relevant areas of comparison and the questions they raise, what kind of contributions may we expect from Egyptian toward the study of AAs (HS) as a whole?

Each of the other branches offers opportunities for the comparison of a number of related languages and the possibility of reconstruction of proto-forms. Egyptian is isolated. There was dialect variation; indeed, it may be more appropriate to term Egyptian a dialect cluster. Information is scanty prior to the Coptic period, but there is some (cf. Edgerton 1951; Edel 1955, 11-12; Vergote 1961). There was definitely difficulty in understanding between speakers from extreme parts of the country, though Edgerton assumes mutual intelligibility among the dialects of all periods. We do not, however, have sufficient knowledge of the dialects, especially of the pre-Coptic period, to reconstruct a hypothetical parent proto-Egyptian. At best we can theorize on what pre-Egyptian may have been (cf. Vergote 1945). (One must also keep open the possibility that Egyptian may be more closely related to some AAs (HS) languages than to others, though this is another matter).

One of the questions which has been raised with regard to AAs (HS) is the feasibility of comparing all of the languages with which we are concerned. Is it reasonable to suppose that the tools we have at hand can be used on languages of such disparate age? This question has often been asked but not satisfactorily answered (cf. Hodge 1968). Indeed, the time span involved is unknown, except that it must exceed 5000 years.

Only one language, Egyptian, has a history approaching 5000 years. In Semitic, Akkadian and Arabic cover the period 2400 B.C. to the present, and one could assume the continuous existence of some forms on a comparative basis. However, only Egyptian offers an observable continuous history. The length of this history has two important aspects. First, the earliest forms of Egyptian give us our earliest examples of AAs (HS), Akkadian running a close second. We have, then, records of something far closer to the proto-language than anything spoken today. Secondly, Egyptian offers us an opportunity to observe, in a partial way, the degree of change which has taken place during this 4500+ year period.

It has often been pointed out that obvious similarities may be deceiving, that

actual etymologies are often hidden under layers of phonetic change, etc. One can have nothing but admiration for the scholars who have been able to reconstruct such submerged etyma (e.g., Leslau 1958). At the other end of the pole, however, are recognizable samenesses which have survived for thousands of years. Egyptian offers us an opportunity to study not only loss but survival.

There are numerous studies on different aspects of Egyptian linguistic history. Vergote's study of the phonology shows the general picture as to the survival of sounds. This is impressive evidence which demonstrates that considerable phonetic similarity survives after 4500+ years. As an illustration of the morphologic and syntactic residue the following list of everyday Coptic expressions has been taken *in toto* from Worrell and Vycichl (1942, 304-305), who quote them "from a manuscript in the possession of the qummuṣ Arsaniyōs" as giving an idea of recent 'speaking Coptic'. The dialect is Bohairic, though not pure Bohairic. The list is in transcription, which must in turn be interpreted into a phonemic system, not here attempted (see Worrell and Vycichl 1942, *passim*). The translations, with a few minor changes, are from the source. These expressions were checked with a view to their retention of earlier features, resulting in the analysis given below. The four lines are: (a) the Coptic data, (b) an attempt to trace each morpheme to its source, giving the earlier Egyptian forms (in transcription) where known, (c) an attempt to date the word formation—in what period was this morphologic structure first found?, (d) an analysis in broad syntactic terms (see below). Notes are added (at the end of this article) to explain some of the choices made and to provide references.

In essaying such an analysis there have been a number of questions which could not be settled with any finality. Some are in dispute, others are due to the rather simple system into which the data were interpreted. While every effort was made to achieve accuracy, there can be no claim to correctness in every detail. Suggestions and corrections will be welcome.<sup>1</sup>

The conventions followed in presenting the list are as follows: Line (a): the Coptic is transliterated, with only a few 'interpretations' (e.g. *w* for *ou* or *u* in some places). Line (b): Old Egyptian (OEg) forms are given in italic capital letters: *HRW*, Middle Egyptian (MEg) in plain capitals: *NQDD*, Late Egyptian (LEg) in italic lower case: *tw*, Demotic (Dem) in plain lower case: *rdt*, Coptic (C) without known history in lower case and parentheses: (*u*), known loans in lower case and brackets: [*arxē*]. The transcription of ancient Egyptian used here varies slightly from the conventional: ' is used for reed-leaf (with ' serving as capital), *č* for *t*, *ğ* for *d*, *x* for *h*, *q* for *k*. The transcription is also spaced for greater legibility. Line (c): Old Kingdom (OK), Middle Kingdom (MK), New Kingdom (NK), but Demotic (Dem) and Coptic (C).

Only major syntactic elements are noted in line (d). Three clause types are distinguished: (1) V (verbal), (2) N N (nominal nominal) and (3) N A (nominal ad-

<sup>1</sup> I am very greatly indebted to Professor J. Vergote, who went over the paper as presented at the colloquium and made numerous corrections. Nearly all of these have been incorporated, but the author assumes full responsibility for any remaining inaccuracies.

verbial). Variations of V used for greater clarity include V Ns (verbal, [nominal] subject), V No (verbal [nominal] object), V A (verbal, adverbial). N d N (nominal, demonstrative, nominal) may be considered a variety of N N but is marked with the d. Nv is used for clauses consisting of a vocative nominal. When two clauses occur, they are separated by /.

- (1) (a) nane pekehow. 'Is your day good?'  
 (b) n ʒ - ' N P ʒ - K - H R W  
 (c) Dem MK  
 (d) V Ns
- (2) (a) nanef. nak hōk. 'It is good. [The same] to you.'  
 (b) n ʒ - ' N - F N - K H ' W - K  
 (c) Dem OK OK  
 (d) V / A N
- (3) (a) aš pe pekrēti. 'How are you?'  
 (b) ʔ X P ʔ P ʒ - K - r d t  
 (c) MK OK MK  
 (d) N d N
- (4) (a) tiwoč pakratiste. 'I am well, my good sir!'  
 (b) t w ' - W Ğ ʒ P ʒ - ʔ - [kratiste]  
 (c) NK MK  
 (d) V / Nv
- (5) (a) šini epekson. 'Give my greetings to your brother!'  
 (b) Š N ʔ ʔ R - P ʒ - K - S N  
 (c) NK NK  
 (d) V No
- (6) (a) tinaše nēi etisxolē. 'I am going to school'.  
 (b) t w ' - N ' ʔ - Š M N - ʔ ʔ R - T ʒ - [sxolē]  
 (c) Dem NK NK  
 (d) V A
- (7) (a) matamoi etekmelētē. 'Show me your lesson!'  
 (b) ʔ M - D ʔ - ' m - ʔ ʔ R - T ʒ - K - [melētē]  
 (c) NK NK  
 (d) V No
- (8) (a) oš mpamtho. 'Read to ('before') me'.  
 (b) ' Š M - P ʒ - ʔ - M T R  
 (c) NK NK  
 (d) V A
- (9) (a) thōm nrōten. 'Shut your mouths'.  
 (b) T M n - R ʔ W - Č N  
 (c) NK NK  
 (d) V No

- (10) (a) ša thnaw ekenkot ō pečhenne. 'Thus far you have been asleep, you loafer!'  
 (b) Š ʒ ' T ʒ - N W K - N Q D D ʒ P ʒ - K N ʒ  
 (c) NK C NK  
 (d) A V / Nv
- (11) (a) arhēts isčen tarxē. 'Begin from the beginning'.  
 (b) ʒ R ʒ - H ʒ T - S ʒ S - N - č ʒ ' - N ʒ T ʒ - [arxē]  
 (c) C C NK  
 (d) V A
- (12) (a) ari wō nēi. 'Answer me!'  
 (b) ʒ R ʒ W ʒ H N - ʒ  
 (c) Dem OK  
 (d) V No
- (13) (a) amu emnēi. 'Come here!'  
 (b) ʒ M W ʒ R - M ʒ N ʒ  
 (c) OK NK  
 (d) V A
- (14) (a) hemsī mmaw. 'Sit down there'.  
 (b) H M S ʒ n - ʒ M - w  
 (c) NK OK  
 (d) V A
- (15) (a) sekōlh hīren pīro. 'Someone is knocking at the door'  
 (b) S T - q l h H R - R ʒ - N ʒ P ʒ - R ʒ  
 (c) NK MK  
 (d) V A
- (16) (a) ačos nēi če nim pe phai. 'Tell me, who is this?'  
 (b) ʒ Ğ D - S N - ʒ Ğ D ʒ N - M P ʒ P ʒ  
 (c) OK OK NK MK OK NK  
 (d) V No / N d N
- (17) (a) anok pe. 'It is I'  
 (b) ʒ N K P ʒ  
 (c) OK OK  
 (d) N d
- (18) (a) ethbe u aki. 'Why have you come?'  
 (b) ʒ R - Ğ B ʒ (u) ʒ R - K - ʒ ʒ T  
 (c) MK C Dem  
 (d) A V
- (19) (a) u pe petekwašf. 'What do you want?'  
 (b) (u) P ʒ P ʒ - N T ʒ - K - W X ʒ - F  
 (c) C OK C  
 (d) N d N
- (20) (a) amu nemēi. 'Come with me!'



- (b)  $\text{ }^{\text{p}}M \text{ }^{\text{p}} \text{ }^{\text{p}}r m - \text{ }^{\text{p}}$   
 (c) OK NK  
 (d) V A
- (21) (a) afthōn petenēi. 'Where is your house?'  
 (b)  $\text{ }^{\text{p}}W - F - \check{C} N \text{ }^{\text{p}} P \text{ }^{\text{p}} - \check{C} N - \text{ }^{\text{p}} W Y$   
 (c) MK MK  
 (d) V Ns
- (22) (a) fhent etianzēb. 'It is near the school'  
 (b)  $F - X N T \text{ }^{\text{p}} \text{ }^{\text{p}} R - T \text{ }^{\text{p}} - \text{ }^{\text{p}} T - S B \text{ }^{\text{p}}$   
 (c) C Dem  
 (d) V A
- (23) (a) awōn mpiro. 'Open the door!'  
 (b)  $\text{ }^{\text{p}}W N m - P \text{ }^{\text{p}} - R \text{ }^{\text{p}}$   
 (c) OK Dem  
 (d) V No
- (24) (a) maštham mpiro. 'Close the door!'  
 (b)  $\text{ }^{\text{p}}M - X T M m - P \text{ }^{\text{p}} - R \text{ }^{\text{p}}$   
 (c) OK Dem  
 (d) V No
- (25) (a) ma peterok. 'Pay what you owe!'  
 (b)  $\text{ }^{\text{p}}M P \text{ }^{\text{p}} - N T \text{ }^{\text{p}} - \text{ }^{\text{p}} R - K$   
 (c) OK C  
 (d) V No
- (26) (a) maron epiēi. 'Let's go home!'  
 (b)  $\text{ }^{\text{p}}M - \text{ }^{\text{p}} R - N \text{ }^{\text{p}} R - P \text{ }^{\text{p}} - \text{ }^{\text{p}} W Y$   
 (c) OK NK  
 (d) V A
- (27) (a) aki ebol thōn. 'Where do you come from?'  
 (b)  $\text{ }^{\text{p}}W - K - \check{H} R - \text{ }^{\text{p}} \text{ }^{\text{p}} T \text{ }^{\text{p}} R - b l \check{C} N \text{ }^{\text{p}}$   
 (c) OK NK OK  
 (d) V A
- (28) (a) wōnh nēi ebol. 'Show me!'  
 (b)  $\check{W} N - \check{H} R N - \text{ }^{\text{p}} \text{ }^{\text{p}} R - b l$   
 (c) NK OK NK  
 (d) V A
- (29) (a) čhisi nteksmē. 'Speak louder ('raise your voice')!'  
 (b)  $\check{C} Z \text{ }^{\text{p}} n - T \text{ }^{\text{p}} - K - S M \text{ }^{\text{p}}$   
 (c) NK Dem  
 (d) V No
- (30) (a) mathebio nteksmē. 'Lower your voice!'  
 (b)  $\text{ }^{\text{p}}M - D \text{ }^{\text{p}} - H B \text{ }^{\text{p}} n - T \text{ }^{\text{p}} - K - S M \text{ }^{\text{p}}$   
 (c) Dem Dem

- (d) V No
- (31) (a) mpermbon. 'Don't be angry!'  
 (b)  $\text{ }^2 M - \text{ }^2 R W - m - B \text{ }^2 N$   
 (c) Dem  
 (d) V No
- (32) (a) wēr te tiačp. 'What time is it?'  
 (b) W R  $T \text{ }^2 T \text{ }^2$  - (ačp)  
 (c) MK OK MK  
 (d) N d N
- (33) (a) ohi mnēi. 'Stand here!'  
 (b) 'H' M  $\text{ }^2 N \text{ }^2$   
 (c) NK MK  
 (d) V A
- (34) (a) ia nekčič ebol. 'Wash your hands!'  
 (b)  $\text{ }^2 \text{ }^2 N \text{ }^2 - K - g \text{ }^2 \text{ }^2 R - b l$   
 (c) NK MK NK  
 (d) V No
- (35) (a) tōnk nšorp. 'First get up!'  
 (b) D W N - K n - X R P  
 (c) NK Dem  
 (d) V A
- (36) (a) maše nak epšōi. 'Go up!'  
 (b)  $\text{ }^2 M - \text{ }^2 M N - K \text{ }^2 R - P \text{ }^2 - x y$   
 (c) Dem OK NK  
 (d) V A
- (37) (a) miok. 'Bravo ('may you flourish')!'  
 (b) M 'R - K  
 (c) OK  
 (d) V
- (38) (a) tinahōl epišnaw. 'I am going to the market'  
 (b) t w ' - N '  $\text{ }^2 - \text{ }^2 l \text{ }^2 R - P \text{ }^2 - (\text{šnaw})$   
 (c) Dem NK  
 (d) V A
- (39) (a) čem nomti. 'Cheer up ('find strength')!'  
 (b) G M  $\text{ }^2 n m t t$   
 (c) NK Dem  
 (d) V No
- (40) (a) ariwi nemēi nuhmot. 'Do me the kindness!'  
 (b)  $\text{ }^2 R \text{ }^2 W \text{ }^2 r m - \text{ }^2 n - W \text{ }^2 - [\text{hmot}]$   
 (c) OK NK NK  
 (d) V No
- (41) (a) phai uatčom pe. 'This is impossible'

- (b)  $P \text{ } ^{\text{3}}$   $W \text{ } ^{\text{c}}$  -  $\text{ } ^{\text{3}}$   $W T \text{ } ^{\text{3}}$  - g m  $P \text{ } ^{\text{3}}$   
 (c) NK NK OK  
 (d) N N d
- (42) (a) anok phōten anok. 'I'm your man ('yours')'  
 (b)  $\text{ } ^{\text{3}}$  N K  $P \text{ } ^{\text{3}}$  -  $\check{C}$  N  $\text{ } ^{\text{3}}$  N K  
 (c) OK MK OK  
 (d) N / N N

This is an effort to identify units across time. Historical linguistics has regularly made such identifications, but there is no clear frame of reference for handling them. They must be viewed both diachronically and synchronically. The word 'morpheme' was used above and would appear at first glance to fit the segments with which we are dealing. Forms such as *f*, *k*, *wr*, *wn*, *sn*, *ǵd* are without question morphemes (in the Bloomfieldian sense) and may be defined as such at any historical level. (As vowels are not present in the older writing, the question of identifying vowel patterns as morphemes across time does not arise). Morpheme identification is, however, complicated by mergers, splits and losses. It is therefore suggested that we call a unit which remains historically recognizable as 'the same' (regardless of splits and mergers) a 'chronomorph'. Where there is no stress on time, the units may conveniently be called 'morphs'. These terms, as well as 'morpheme' where appropriate, will be used in the following discussion. The adoption of the word 'chronomorph' does not, of course, solve the basic problem of the frame of reference. This topic, the historical interrelationship of syntax and morphology, will be taken up in a later article.

Even a brief examination of the above analyses reveals a large proportion of survivals from the early periods. In order to make this somewhat more meaningful, without making statistical analyses, the OK and MK forms were abstracted from line b. These are given in the table below, with the Coptic forms found in the sentences. The correspondence between the early and later forms has been roughly classified in regard to closeness of form and meaning. A + means a clear correspondence, (+) indicates significant change but that the correspondence is still very good, a — indicates that the connection is less clear. These evaluations are admittedly subjective, and some of the etymologies are disputable, but the overall picture seems clear. Where the same OEg or MEg morpheme serves as the basis for several forms, these are all given under one number (e.g. 13, 14, 24). In some cases two or more chronomorphs are involved in a single form (e.g. *ariwi*, where a plural *-w* is added). Such formatives are not treated separately here, even though they would add to the figures below.

	Egyptian	Coptic	Form	Meaning	Gloss
(1)	'	i	—	+	'me, my'

		zero	—	+	
(2)	'	ō	—	+	'oh'
(3)	''t	i	—	+	'come'
(4)	''	ia	—	+	'wash'
(5)	'w	a	—	(+)	'is' (particle), C (verb pre- fix)
(6)	'wt'	at	(+)	+	'not being, which not'
(7)	'm	mp	(+)	+	'do not' (neg. imper.)
(8)	'm	ma	(+)	+	'give' (causative impera- tive)
(9)	'm	mmaw	(+)	+	'there'
(10)	'mw	amu	+	+	'come'
(11)	'n	ni	(+)	(+)	(particle)
(12)	'nk	anok	+	+	'I'
(13)	'r	e	—	+	'to'
		ero	+	(+)	
(14)	'r	a	—	—	'do' C (verbal prefix)
	'r'	ar, ari	+	+	'do'
	'r'w	ariwi	+	+	'do!' (pl.)
	'r	r	(+)	(+)	'do' C (in compound)
	'rw	er	+	+	'do' C (part of neg. imper. prefix)
(15)	'x	aš	(+)	+	'what?'
(16)	's	is	+	(+)	(particle) C (in compound)
(17)	'wy	ēi	—	(+)	'room, house'
	't(?)	a	—	(+)	
(18)	'm	amo	(+)	(+)	'know' C (in compound)
(19)	'n	an	(+)	(+)	'pleasing' C (in comp.)
(20)	'h'	ohi	(+)	+	'stand'
(21)	'š	ōš	+	+	'speak loudly'
(22)	w <sup>2</sup> h	wō	(+)	—	'set down' C 'answer'
(23)	w'	u	(+)	(+)	'one' C 'a'
(24)	wn	wōn	+	(+)	'open'
	'wn	awōn	+	+	'open!'
(25)	wr	wēr	+	+	'how much?'
(26)	wx <sup>3</sup>	waš	(+)	+	'seek'
(27)	wğ <sup>3</sup>	woč	(+)	+	'be healthy'
(28)	b'n	bon	+	(+)	'evil' C 'anger'
(29)	p <sup>3</sup>	p, pi	(+)	(+)	'that' C 'the'
	p <sup>3</sup>	phō	(+)	(+)	'that' C 'the one'
	p <sup>3</sup>	phai	(+)	+	'this'
(30)	p'	pe	+	+	(demonstrative)

(31)	f	f	+	+	3 sg. m.
(32)	m	m	+	+	'who?'
(33)	'm	m	+	+	'in'
(34)	m'n <sub>3</sub>	mnēi	+	+	'here'
(35)	m'r	mio	(+)	+	'flourish'
(36)	mtr	mtho	+	+	'be present' C 'presence'
(37)	n	n, nē, na	+	+	'to'
	n'	n, en	+	+	'of'
(38)	n <sub>3</sub>	n	(+)	(+)	'those' C 'the' (pl.)
(39)	n''	na	(+)	—	'travel' C (future particle)
(40)	nw	naw	+	+	'time' C 'time, hour'
(41)	nqdd	enkot	+	+	'sleep'
(42)	nt'	ete	(+)	+	'which'
(43)	r'	ro	+	+	'mouth, door'
	r'w	rō	+	+	'mouths'
(44)	hb'	hebio	+	+	'below' C (here in comp.)
(45)	hrw	how	(+)	+	'day'
(46)	h <sub>3</sub> t	hēt	(+)	+	'front, beginning'
(47)	h'w	hō	(+)	(+)	'limbs', C 'self'
(48)	hms'	hemsī	+	+	'sit'
(49)	hr	h	(+)	(+)	'face', C (here in comp.)
		hir	+	+	'upon'
		zero	—	—	'upon'
(50)	xnt'	hent	+	+	'be in front, facing'
(51)	xrp	šorp	+	+	'lead, be first'
(52)	xtm	štham	+	+	'seal, shut'
(53)	s	s	+	+	3 sg. f.
(54)	st	se	(+)	+	3 pl. c.
(55)	sb <sub>3</sub>	zēb	(+)	+	'teach', C (here in comp.)
(56)	sm'	smē	+	(+)	'announce', C 'voice'
(57)	sn	son	+	+	'brother'
(58)	š <sub>3</sub> '	ša	(+)	(+)	'begin', C 'as far (as)'
(59)	šm	še	(+)	+	'go'
(60)	šn'	šini	+	+	'question, inquire'
(61)	k	k, ek	+	+	2 sg. m.
(62)	kn'	čhenne	(+)	+	'be sullen(?)', C 'loafer'
(63)	gm'	čem	+	+	'find'
(64)	t <sub>3</sub>	t, th, ti	(+)	(+)	'that', C 'the'
(65)	t'	te	+	+	(demonstrative)
(66)	tm	thōm, tham	+	+	'close'
(67)	čn	ten	(+)	+	2 pl.
(68)	čn'	thōn	(+)	+	'where'

(69)	čz'	čhisi	+	+	'lift'
(70)	d'	t	(+)	+	'give, cause'
(71)	dwn	tōn	+	+	'stretch out, straighten'
(72)	ǵbǵ	thbe	(+)	(+)	'replace'
(73)	ǵd	če	(+)	(+)	'say', C 'that' (conj.)
	'ǵd	ačo	(+)	+	'say!'

Using the highest rating where there are several (e.g. 13, 14), we find 31 of the 73 chronomorphs have correspondences rated + +. Of the remaining 42, 20 have a (+) + rating, 4 a + (+), while 10 have (+) (+). This leaves only eight with very weak ratings.

It is true that a considerable proportion of the above list consists of very short morphs. It is also impossible to estimate with any high degree of accuracy the actual changes and survivals in phonetic terms. However, a rough estimate can be made, and for some phonemes near identity may be assumed (e.g. *m*, *n*, *s*). Others are very close (e.g. *p*, *t*, *k*). In the above list, considered in gross phonetic terms, we find 24 cases where there are at least two consonants close enough to be called the same for both stages (12, 14c, 24, 25, 28, 34, 36, 40, 41, 44-46, 48-52, 56, 57, 60, 66, 71). Another eight have at least two corresponding (but not identical) consonants (26, 27, 55, 62, 63, 67-69, 72). If we consider the fact that initial glottal stop was written in hieroglyphs but not in Coptic (though phonetically there) and that ' became ', we may add about a dozen more (6, 9, 10, 13, 14be, 15, 16, 18-21, 73). So even eliminating very short morphs and counting only two consonant survivals, we still have well over half the list (about 43 of the 73).

This is only a sample, and too much in the way of generalizations should not be made on the basis of it. One may note, however, the conservatism of the pronouns, a morphological group which has been used as a preliminary comparative device (e.g. Tucker 1967).

Returning to consideration of the sentences and their analysis, we find that the syntactic patterns shown in line (d) may all be found in OEg. This may have little significance, as the patterns indicated are very broad. There could well have been more syntactic analysis between the 'words' of lines (a) and (b) and the broad terms of (d). For this to be truly revealing, however, one would need syntactically comparable OEg, MEg, LEg and Dem versions of each of the sentences. The most serious question regarding such analysis is the extent to which the syntactic structures are unique to AAs (HS). If we find the same etyma in the same syntactic relation in two or more languages, this is obviously significant. If, however, the syntactic relationship is one found far more widely than the family under study, only the etyma retain their full significance.

It was earlier mentioned that there has arisen something of a dichotomy between the advocates of a 'vocabulary' approach and those favoring morphology as a safer test of relationship. It would appear that the chronological analysis of the above

sentences may help clarify this issue. If we examine lines (b) and (c) above, we are faced with a considerable difference between the dating of individual chronomorphs (b) and the dating of the structures into which they combine (c). The survival table above was calculated in terms of the chronomorphs only. It is very clear from line (c) that recombination is constantly going on and that while there are some amazing survivals of OEg structure, we are faced with a radically revised morphology (OK-28, MK-14, NK-36, Dem-15, C-8). We therefore conclude that, aside from the overall sentence structure (d), the chronomorphs are the most conservative elements and therefore the ones on which we must most heavily rely for comparative purposes. This choice dissolves the distinction between a vocabulary approach and a morphological approach, as a word may represent a survival as readily as may an affix. We have, as for phonemes, mergers, splits and loss of morphemes historically, but the recognition of the chronomorph as the primary unit gives credit to the advocates of both hitherto opposing positions. This is not to deny validity to the morphological approach. It means rather that morphology as such is likely to have a lesser time depth. It is highly significant for closely related languages, far less so for those separated for long periods.

The showing as a whole is impressive. A really meaningful proportion of chronomorphs is identifiable after 4000-4500 years. If such a large percentage demonstrably survives in Egyptian, is it not reasonable to assume that other languages also have retained sufficient of their lexicon for comparison to yield results of some linguistic consequence?

#### NOTES ON SENTENCES 1-42

These give references which will, hopefully, enable the non-Egyptologist to see the basis for the above decisions of origin and date. Unfortunately some of them are difficult to use without a knowledge of hieroglyphs.

- (1) (b) *nʒ*-. Steindorff derived this from *wn* (so still 1951, 136), but this was disputed by Spiegelberg (1925, 60). Erichsen still supports the *wn* origin (1954, 88, 202), as does Westendorf (1967, 123). One would think a derivation from *n*' 'pertaining to' also possible. The derivation *nʒ*-'*n* here is from Spiegelberg (1921, 77).
- (2) (b) *h'w* Erman and Grapow (1957, III, 37). Derivation of *hō*-, Spiegelberg (1921, 226).
- (3) (b) 'x. Definitely MEG (Gardiner 1957, 408), though Kasser refers the interrogative only back to LEG (1967, 69), giving 'xt 'thing' as the probable origin. The probability is that it is the same as the hortatory 'x (Edel 1964, 433) and therefore OEg.  
*p*'. Westendorf takes *pe* from LEG *pʒy*, querying the further derivation from *pw* (1967, 144, with reference to Fecht). This scepticism is justified as

regards *pw*, which is from a different formation. The OEg forerunner of *pe* is *p'* (cf. Edel 1955, 11-12; 1964, 489). The historical sequence is OEg *p'* LEg *p₃y* C *pe*.

*rdt*. Erichsen *rtt* (*rdt*) (*rd.wj.t*) with reference to Coptic but no etymology (1954, 258).

- (4) (b) *tw'*. Dyn XVII (Gardiner 1957, 98; Lefebvre 1955, 59-60).
- (c) *p₃'* 'my', article plus first person. Note the vocative with *pw*, *pn* in OEg (Edel 1955, 86) and MEg (Gardiner 1957, 87). These are postpositive but reflect the same use. Prepositive *p₃'* with vocative is found in LEg (Erman 1933, 78). For vocative noun phrase before or after other clauses see Edel (1964), 435.
- (5) (c) Though *šn'* is attested only back to MEg, the morphological structure is, strictly speaking, OK. However, the form in 5a is not the OEg imperative, which Coptic rarely retains (ex. 12, 13, 16, 23, 25) but the infinitive, which Coptic regularly uses for the imperative (so 8, 9, 14, 28, 29, 33, 34, 39). This usage is NK or later (cf. Erman 1933, 164), hence NK in line (c). Such a shift of function rather than form is not well handled by the broad classifications of the present analysis. One could argue for OK in line c on the basis of form alone.  
*'r-p₃-k-sn*. As a construction this is NK (cf. Erman 1933, 299-302). As construed with this verb it may be later (cf. Erichsen 1954, 514; Steindorff 1951, 189).
- (6) (b) *tw'-n''*. Gardiner derived *tina*-V from LEg *tw' m n''* (')*r 'r(t)*. However, the form as we have it is late, first found in the Roman period (Williams 1948, 227). See also Steindorff (1951), 145; Westendorf (1967), 116 and references.  
*n'* 'with reference to me' (dativus ethicus) Erman (1933), 291; Erichsen (1954), 505.  
*'r-t₃-*. For *šm* with *'r-t₃* see Erichsen (1954), 505. *'r* as 'to' is, of course, OK, but *'r-t₃-N* is later. *t₃* is earliest attested in the First Intermediate Period (Edel 1955, 88). On MK use of *p₃*, *t₃*, *n₃* as articles see James (1962), 107-108.
- (7) (b) *tamo* is generally derived from *d't-m* (so Spiegelberg 1921, 145 with query; Erman 1933, 131-132; Erman and Grapow 1957, I, 184.22 without query). *'m* is both LEg and Dem (Erichsen 1954, 60). Earlier *'m* is 'swallow'; see also Erman (1933), 120.
- (c) Dating *'m-d'-m* as a construction is difficult. *rd'* plus verb is OEg (Edel 1955, 221, with reference to Coptic causative *t-*). Here, however, we have the imperative of *d'*, i.e. *'m*, followed by *d'* plus verb. For Dem causatives in *t* see Lexa (1947-1951), 340-342. Cf. item 30.
- (8) (c) For LEg *m mtr* see Erman and Grapow (1957), II, 172.1; for Dem *n p₃ mt* see Spiegelberg (1921), 65. The noun meaning 'presence' is NK but related



- to an OK verb 'to be present, witness to' (Erman and Grapow 1957, II, 171; cf. Westendorf 1967, 103).
- (9) (b) *tm* in sense of 'shut (the mouth)' already Pyr 230 (Faulkner 1962, 298). For the use of *n/m* before the direct object in Dem see Spiegelberg (1925), 115. For its lack in LEg see Erman (1933), 296-297. It is probably from an OEg morpheme. Some take it from the nominalized preposition *n*' (e.g. Till 1961, 126), others from the preposition *m* (e.g. Westendorf 1967, 115). I would rather reserve judgment at present.
- (10) (b) *ekenkot* is here treated as first present. Were one to interpret it as circumstantial (cf. Mallon 1926, 110), the *e-* would derive from <sup>2</sup> *W. nqdd* is MEg (Erman and Grapow 1957, II, 345.1). There is an OEg *qdd* (Edel 1955, 194).
- (c) *šj' r* is MEg (Gardiner 1957, 135), *r šj'* and *šj'* LEg (cf. Erman and Grapow 1957, IV, 408.11).
- (11) (a) Coptic *ar-* here, in 12 and 40 is historically an imperative.
- (b) *'s-n-čj'-n*. The first *n* is lost. Vergote derives *is-*, hitherto unexplained, from *'s* (Erichsen 1954, 70), which is probably OEg non-enclitic *'s* (Edel 1964, 430), Coptic *eis* (Crum 1939, 85). I take the first *n* to be the preposition used in a temporal sense (Edel 1964, 388). For *žen* see *n čj' n* 'seit' (Erichsen 1954, 667). So Spiegelberg (1921), 272.
- (c) *'r' hjt* in the sense 'get in front of' is OEg (Erman and Grapow 1957, III, 21.11). It later has the meaning 'regret' (Dem, Erichsen 1954, 290). The construction here is apparently Coptic (not in Crum).
- (12) (b) For *wjh* rather than *wšb* see Erichsen (1954), 77 (cf. Lexa 1947-1951, 599.7).
- (c) The forms are all OEg, but the combination is Dem. The position of *n*' would not be normal OEg or MEg if *wjh* is considered the noun object, but see Edel (1964), 437. For dative after noun in LEg see Erman (1933), 347.
- (13) (b) *emnēi* = *'r-bw-nj'*. The MK writing of *bw-nj'*, *m'nj'*, has *m*, not *b*. The difference may be graphic or dialectal. Edel has a reasonable etymology of *m'nj'* which would break this into several chronomorphs: *mr nw j'* 'like this, hey!' (see 1964, 386). See Erichsen (1954), 113; Spiegelberg (1921), 60; (1925), 177; Erman (1933), 287; Westendorf (1967), 95. Crum gives *mnai* 'here' and *mnē* 'there' (1939, 174). One must consider whether *eta* in our source, here transcribed *ē*, is not sometimes [a] (see Lambdin 1958); see items 12, 16, 20, 28, 33, 40.
- (14) (b) OEg *'m* = *mmaw* Edel (1964), 384. The form with *w* is apparently Dem. For LEg forms see Erman (1933), 285-286; Spiegelberg gives LEg *n-'m* (1925, 177). Cf. Westendorf (1967), 106.
- (15) (b) For *st* see Gardiner (1957), 46; Lefebvre (1955), 60. As prefix it must be assigned to NK, but the form itself is probably OK. The transcription as MEg is a compromise.

- kōlh* from Dem *qlh* (Westendorf 1967, 63).
- (c) One may contrast the first present *f-* (22), apparently Coptic, and the first present *ti-* (4) and *se-*, clearly LEg. *hr r' n' p' r'* is all OEg except for the use of *p'*.
- (16) (b) *'gd-s*. For Dem *'gd st* see Lexa (1947-1951), 455.  
For *p' = phai* see Edel (1955), 87.
- (c) *'n-m*. On MK use see Gardiner (1957), 406.  
The form *p'y* with *-y* is NK (Erman 1933, 53); cf. Westendorf (1967), 122.
- (17) (b) This exact expression occurs in OEg (Edel 1964, 489).
- (18) (b) *'r-ḡb'* Westendorf (1965, 41 et al.).  
*'r-k-*. On the older Egyptian forms of the Coptic verb prefixes see Polotsky (1944). For this form contrast Edgerton (1935), 257-261.
- (19) (c) For LEg parallels to *p'-nt'-k-wx'-f* see Erman (1933), 433 (cf. Spiegelberg 1925, 244-245; Lexa 1947-1951, 847 [references only]).
- (20) (b) *'m' = amu*. An old imperative; see Edel (1955), 296; Westendorf (1965), 5.  
*'rm-*. Erman (1933), 309; Westendorf (1967), 93. The latter has references to further possibilities; see especially Edel (1967).
- (21) (b) *'w-f-*. For MEg parallels see Gardiner (1957), 408.12; for LEg see Erman (1933), 377.  
*p'-čn-N*. For MK date see Gardiner (1957), 87.  
*'wy*. Earlier considered to be from *'t*; see Westendorf (1965), 46 and references.
- (22) (b) *'t-sb'*. So Westendorf (1965), 8; Spiegelberg (1921), 7; Erichsen (1954), 51.  
Better *'wy-n'-sb'*? Kasser (1964, 28) takes *an-* from *'* 'arm, side' plus *n'* 'of'. It is better taken as 'house of teaching' with the *-n-* from *n'* 'of'.
- (23) (a) OEg shows the *'*- augment on many verbs (Edel 1955, 291), here clearly reflected in the *a-*. This is a historical imperative (Till 1961, 151), hence OK in line (c).
- (25) (c) It would appear probable the *p'-nt'-r-k* goes back at least to Demotic, but I have not been able to trace it.
- (26) (c) *'m-r'-n*. Imperative plus prospective *sḡmf. rd'* plus *sḡmf* is OK, but this particular sequence is not datable to this period (cf. Edel 1955, 221-222; Gardiner 1957, 370; Lefebvre 1955, 188, 344-345). For LEg see Erman (1933), 137; Dem, Spiegelberg (1925), 84-85. Cf. Till (1961), 157; Mallon (1926), 124.
- (27) (b) *hr* lost completely.
- (28) (b) *wn-hr*. Erman and Grapow (1957, I, 312-313), attested MK; this meaning NK. Cf. Erichsen (1954), 92. The use of the infinitive as imperative would also lower the date.
- (29) (b) *sm'*. This assumes a connection between C *smē* 'voice' and MEg *sm'* 'report' (cf. Erman and Grapow 1957, IV, 127-128; Faulkner 1962, 227).
- (30) (b) The dating of *hb'* is uncertain, depending on whether one identifies it

with one of the earlier attested words with these consonants (see Erman and Grapow 1957, II, 485-486; Faulkner 1962, 157-158). On *thebio* see Edel (1955), 221; Till (1961), 139. We are, unfortunately, often unable to judge the presence of polysemy in ancient Egypt. In this instance we do not know whether *hb* 'to plow', *hb* 'to humiliate(?)', *hb* 'to be low' are the same word. Cf. Ullmann (1966), 232.

- (31) (b) *'m-r*. See Lexa (1947-1951), 466.
- (32) (b) *wr* MEg (Gardiner 1957, 408). The Demotic word taken to be the origin of Coptic (*a*)*ḡp* by Sethe and given with a query by Spiegelberg (1921, 274) is now interpreted as a spelling of *wnwt* (Erman and Grapow 1957, I, 316; 5.557; Erichsen 1954, 90).  
*t*' is the origin of *te*, as *p*' of *pe* (see 3b above).
- (34) (b) *gḡ* certainly Demotic (Erichsen 1954, 595). Possibly NK; so Spiegelberg (1921), 282 on the basis of the personal name *nḡmgḡw*(?) (see Ranke 1935, 215.20).
- (35) (b) *xrp* 'be first'.  
(c) *n xrp* Erichsen (1954, 367.)
- (36) (c) While *'m-šm* has OEg forms the use of this combination as imperative rather than causative is late. (Dem - Lexa 1947-1951, 455).
- (38) (b) *hl* from OK *hr* 'sich entfernen' (Erman and Grapow 1957, III, 144), later (NK) 'nach oben entfernen' (Erman and Grapow 1957, III, 146), see Erichsen (1954), 327. One could transcribe *HR*.
- (39) (b) *nmtt* Dem in sense of 'strength'. Erichsen (1954, 220) suggests a connection with *nmtt* 'stride' (Pyr. on); so also Westendorf (1967), 123.
- (40) (b) *hmot* identified with Heb. *hemed* by Spiegelberg (1921, 235).
- (41) (b) *gm* 'strength' Dem - Erichsen (1954, 580).
- (41) (c) *w'-wt'-gm* is hard to date as a construction. *'wt'* + N is good OEg (Edel 1964, 554). The use of *w'* as indefinite article is attested in Papyrus Westcar (Lefebvre 1955, 108), and so the construction as a whole is here dated NK, stressing the Hyksos date of the MS rather than its primarily MEg language. NK also reflects the use of *w'* as 'a' not followed by *n* 'of'.
- (42) (c) For the *p<sub>2</sub>-čn* construction in LEg see Erman (1933), 78.

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## DISCUSSION

BARR: Professor Hodge's paper contains a number of very interesting remarks about general methods of procedure in our whole subject, including discussion of things like comparative syntax, comparative semantics, and the rôle which they may play.

PETRÁČEK: The paper is extremely interesting from the point of view of methodology, but it rests on a particular linguistic situation, namely that of a language which was written during several millennia. We can of course find similar cases in the Semitic field, but in Cushitic or Berber for instance the socio-linguistic situation is altogether different with regard to writing. I therefore ask a methodological question. Before one can use our colleague Hodge's method, must one not solve the socio-linguistic question, that is the question of the functioning of a written language as against an unwritten language in society?

MARCEL COHEN: The previous speaker has rightly pointed out that it is not sufficient for a language to have been written, even for thousands of years, for us to be completely enlightened concerning it. We have in the Romance languages, derived from Latin, what I believe to be a unique example. Because there we have Latin, which was a written language and which we know well, which has continued to be understood and, on the other hand, which has given birth to languages which differ both from each other and from it considerably. Well, Romanists have been obliged to come to the concept of Romance, or of proto-Romance, which is different from Latin. And I would like to mention just one fact, that if one were to consider only the Romance languages one could not guess that Latin had a synthetic participle with special endings which distinguished it from the active conjugation. We have there a good example of the caution with which one has to look upon all reconstructions and a good example also of the complexity of the way in which languages evolve.

HODGE: There were two questions raised I believe: one was whether or not it was possible to talk about this in general terms, both with regard to written and unwritten languages. Strangely enough I believe that language changes no matter whether it is written or not, that is: we simply assume that language goes on. Obviously any written language is only a very very partial reflection of what is actually said, so that the effect of the written language is more on the written language than it is on the spoken language. So if we take the two ends of the spectrum, earliest Egyptian and then Coptic, we see that we have a vast change. I do not mean to imply though that the history of Egyptian is, say, the history of Berber but only that you have had change and that that change can be as great in both of these fields. I would like to point out that what I have said here is very relevant to these questions of vocabulary

versus morphology and so forth, and that if we take say such a basic thing as prefixes in Semitic and in Hamito-Semitic generally we would think that Semitic is characterized by say a prefixal conjugation. We know that it has a prefixal conjugation, and yet in the entire history of Egyptian we have no prefixal conjugation, and yet at the same time no one doubts that they are related. I think that facts like these are very interesting ones.

MARCEL COHEN: This is a question which interests the whole of Hamito-Semitic and indeed the evolution of languages in general. In so far as we are able by means of comparison to reconstruct a picture of the conjugation of the verb in proto-Hamito-Semitic, we find a system in which prefixes play a principal part. But the first written of them (along with Akkadian) is Ancient Egyptian, and from the earliest Egyptian documents onwards we find only insignificant traces of the prefix conjugation. These insignificant traces I believed to have detected in what one calls the participle and this has met with general approval. But the Egyptian conjugation throughout the whole long period of its evolution is constructed differently, namely by means of suffixes added to what we presume to have been nominal forms. One must therefore assume—if our reconstruction is correct—that the oldest Egyptian situation already represents the result of a long period of evolution with deterioration of the ancient conjugational pattern. This is already an extraordinary fact. But it so happens that within Semitic, after the millennia of Ancient Egyptian survival, we find that in the relatively recent past Aramaic, which had a conjugation very similar to that of Hebrew and Arabic (which have only prefix forms), has in certain points arrived at almost exactly the same state as Ancient Egyptian, namely a new conjugation without prefixes and where the surviving opposition which appears to be basic between perfective and imperfective is expressed by means of nominal forms supplied with suffixes. We see there, if we interpret the situation in this way, a recurrence after millennia of an almost analogous evolutionary process. This is a topic for reflection I think for all us Hamito-Semitists and for a long time to come.





## LA POSITION INTERMÉDIAIRE DE L'ANCIEN ÉGYPTIEN ENTRE L'HÉBREU ET L'ARABE

J. VERGOTE

Dans le domaine de l'indo-européen et des langues romanes, on détermine la parenté et le degré de parenté entre les différents idiomes en se fondant essentiellement sur les faits de vocabulaire. Chacune de ces langues a tellement modifié son système morphologique qu'il existe peu de points de comparaison entre elles ou que, tout au moins, ceux-ci n'interviennent guère dans l'établissement des généalogies. Ceci fut particulièrement bien illustré par l'observation suivante de Bally : « L'existence de l'opposition *il est* : *ils sont* est une des meilleures preuves que notre langue remonte à l'indo-européen ...; mais rien n'est plus étranger au système verbal actuel [du français] que la conjugaison du verbe *être* » (1944, § 14).

Lorsqu'on compare l'ancien égyptien avec les langues chamito-sémitiques, on constate que la morphologie offre, ici aussi, peu d'arguments en faveur de leur parenté. Le fait fut clairement mis en évidence par Thacker (1954). A l'exception du pseudo-participe ou 'old perfective' égyptien et du permansif-parfait sémitique, notamment en akkadien, le système verbal égyptien se montre parfaitement indépendant.

Au contraire, les enquêtes de F. Calice et de M. Cohen, fondées sur de nombreuses recherches antérieures, en particulier sur celles d'A. Ember, ont nettement établi la parenté entre ces parlers sur la base d'une grande quantité de correspondances lexicales. Nous croyons avoir démontré l'existence d'un lien de parenté plus étroit entre l'égyptien et les langues sémitiques en faisant observer que la première langue a en commun avec les autres la grande majorité des 'structural patterns' ou schèmes de ses sémantèmes (ou lexèmes). En nous limitant aux types structurels créés par changement interne, nous avons dénombré, en effet, 60 schèmes différents dont le sens de classe ou 'class-meaning' est identique ou apparenté à celui d'un type proto-sémitique. Il faut compter 46 schèmes si l'on fait abstraction des cas qui entrent deux ou trois fois dans ce calcul, p. ex. parce qu'ils créent en même temps des substantifs et des verbes ou des substantifs et des adjectifs. Cela dénote, à notre avis, une dépendance suffisamment grande pour qu'on puisse en conclure que l'égyptien est une langue sémitique à part entière (Vergote 1965).

Nous voudrions examiner ici les résultats de ces recherches en ce qui concerne les rapports de l'égyptien, d'une part avec l'hébreu, d'autre part avec l'arabe.

Trois facteurs ont déterminé le développement propre de la langue de l'Égypte et ont modifié la structure protosémitique d'un certain nombre de schèmes égyptiens.

(1) La loi selon laquelle la voyelle de la syllabe accentuée est longue lorsque celle-ci est ouverte, brève lorsqu'elle est fermée.

(2) La loi de la pénultième, ainsi appelée d'après la dénomination *Zweisilbengesetz* de son inventeur Fecht (1960). Dans l'égyptien classique, l'accent du mot ne peut, en règle générale, pas remonter au-delà de la pénultième syllabe du mot. Les schèmes protosémitiques se sont conformés à cette loi grâce à la syncope de la voyelle atone se trouvant en syllabe ouverte après l'accent. On remarquera que, de l'hébreu et de l'arabe, seule la première langue connaît une loi de limitation de l'accent ainsi que la chute de la voyelle ouverte atone après l'accent.

(3) La terminaison *-u*, qui se rencontre, en protosémitique, dans tous les substantifs, adjectifs et noms verbaux, est tombée en égyptien excepté dans 10 schèmes de substantifs masculins. Dans les substantifs féminins, la terminaison *-atu* subsistait dans l'état le plus ancien de la langue, mais le *-u* a disparu avant la fin de l'Ancien Empire. Quant aux noms masculins précités, on constate que dans 8 cas sur les 10 la terminaison sert à caractériser des déverbatifs comme substantifs, ceci dans le sens de 'quelqu'un qui/ce qui est ou fait telle ou telle chose'. Il semble en résulter que la terminaison protosémitique *-u* a pris ici une valeur nouvelle : elle est devenue un morphème de transposition ou de translation, dans l'acception que resp. Bally (1944) et Tesnière (1959) ont donnée à ces termes. Les deux cas restants sont des noms collectifs ou plutôt des noms du grand nombre : l'égyptien les a donc assimilés aux autres et les a dotés de cette marque substantivale. Contrairement à ce qui s'est fait dans Vergote (1965), le paradigme *qtl* 'tuer' sera réservé ici aux schèmes protosémitiques et la terminaison *-u* y sera partout notée; le paradigme *sdm* 'entendre', avec ou sans *-u*, caractérisera les schèmes propres à l'égyptien.

Rappelons qu'en hébreu le *-u*, comme les autres voyelles finales brèves, a partout disparu; il s'est partout conservé en arabe classique.

L'interférence des trois facteurs précités est telle que sur les 60 schèmes égyptiens la quantité de la voyelle accentuée protosémitique s'est modifiée dans 20 cas; ils sont en général notés par B 1, 2, etc. dans les listes de Vergote (1965). Dans 40 cas, cette quantité vocalique est demeurée inaltérée; ils sont pour la plupart notés par A 1, 2, etc. et C 1, 2. Ne pouvant reproduire ici pour tous les schèmes les correspondances entre l'égyptien d'une part, l'hébreu et l'arabe d'autre part, nous nous limitons à quelques exemples particulièrement représentatifs.

A. Schèmes ayant conservé la quantité vocalique du protosémitique.

A 1. *qattālu* : *saḏḏāmu*, nom d'agent.

H. *gannāḥ* 'voleur'; *ṭabbāḥ* 'boucher'; *dajjān* 'juge'; *rakkāḥ* 'cavalier';

Ar. *naḡḡār* 'charpentier'; *kallāb* 'éleveur de chiens'; *fallāḥ* 'laboureur';

Copte-boh. *akhō* : ég. *ḥakkāzu* 'magicien'; c. *sanūth* : ég. *sannādu* 'poltron'; c. *ekōt* :

ég. *yaḳḳādu* 'constructeur'; ég. (*Ap*)-*εμσυννις* : *massānu* '(Horus) le harponneur'.

F. *qattālatu* : *saḏḏāmat* très pauvrement représenté en hébreu et en arabe :

H. *kappōreṭ* 'couverture'; Ar. *naššāsa* 'chasse-mouches' (exemple donné par Vycichl);

C. *satō* : ég. *saddāzat* 'éventail'; c. *e/ačō* : ég. *taẓẓāyat* 'pince'; c.-boh. *a/ečō* : ég. *daẓẓāyat* 'vipère' [litt. 'ce qui tremblotte, saisit, rampe'].

A 3. *qatūlatu* : *saḏūmat* nom abstrait de qualité et d'action.

H. *gēbūrāh* 'puissance'; *'emūnāh* 'fidélité'; *mēlūḳah* 'royauté';

*nēbū'āh* 'prophétie'; *jēšū'āh* 'aide'; *'arūḳāh* 'guérison';

Ar. *ḏarūra* 'besoin' (exemple très rare);

C. *kmēme* : ég. *kamūmat* 'obscurité'; c. *ašē* : ég. *'ašūzat* 'multitude'; c. (*a*)*mēe* : ég. *maẓū'at* 'vérité';

C. *špēre* 'prodige' : ég. *ḥapūrat* 'événement'; c. *crēce* 'dot' : ég. *garūgat* 'équipement'; c. *e-tbē't* 'à cause de' : ég. *ir-ḏabūzat* 'en échange'.

A 4. *qatūlu* : *saḏūmu* adjectif et participe passif.

H. *'āṣūm* 'fort'; *'ārūm* 'astucieux'; *bāṭū<sup>ah</sup>* 'confiant';

*qāṭūl* 'tué'; *lāḥūš* 'habillé'; *šāḳūn* 'habitant';

Ar. *'arūs* 'jeune marié (*Brāutigam*) [homme joyeux]'; *rasūl* 'prophète [homme envoyé]'; *farūq* 'très peureux'; *ḡahūl* 'très ignorant'; *kasūl* 'très lent';

C. *ebyēn* 'homme pauvre, misérable' : ég. *bayūnu* 'id.';

boh. *swēn* 'personne très connue, célèbre'; c. *prēš* 'chose étendue, natte'; *awēt* 'communauté, monastère [ce qui est séparé]' : ég. *sawūnu*, *parūšu*, *awūdu* 'quelqu'un qui est connu, étendu, séparé'. Tous les anciens participes sont substantivés, comme en arabe.

A 10. *qālqalu* : *sāmsam* nom concret.

H. *kôḳāḥ* < *kaukab* 'étoile'; *galgal* 'Roue'; *dardar* 'épines';

Ar. *ḏakḏak* 'surface unie'; *qarqar* 'sol uni'; *dabḏab* 'timbale';

C. *čōč* : ég. *ḏāẓḏāz* 'tête' (exemple unique).

F. *qālqalatu* : *samsāmat*;

H. *kalkālāh* 'corbeille'; *qašqéšeṭ* 'écaille';

Ar. *qamqama* 'bocal';

C. *kelkōle* 'pustule'; *mehmūhe* : ég. *maḥmāḥat* 'pourpier'; boh. *čičhōy* < *\*tayṭāyat* 'tresse'.

En égyptien, le type *qālqalu* : *sāmsam* a servi surtout à créer des verbes dénotant la répétition, l'intensité de l'action. Il en est de même pour les schèmes *qilqilu* : *sīmsīm*, *qatāltalu* : *saḏāmḏam*, *qatīltilu* : *saḏīmḏīm*.

C. *hoth<sup>et</sup>* : ég.-dém. *ḥāṭḥat* 'fouiller, examiner'; c. *towte* : ég. *tāwtaw* 'rassembler, etc.'.

B 9. *qatilu* : *saḏīmu* nom du grand nombre.

H. *šāmīr* 'épines'; *\*šārīr* 'muscles du ventre'; *šānī* 'cramoisi et objets teints au cr.';

C. *spīr* : ég. *sapīru* 'côte'; *skīm* : ég. *sakīmu* 'cheveux gris'; c. *trim* 'trèfle'; c. *hyīb* 'agneau'.

Dans Vergote (1965), ce schème fut classé sous B pour faire suite au type *qatīlu* : *saqīm* adjectif et participe passif. Il désigne des plantes, des animaux, des objets qui se présentent généralement en groupes. Le même sens de classe appartient, en hébreu et en égyptien, au schème A 9 *qīlu* : *simu*. Il ne semble pas être représenté en arabe, sauf peut-être dans le mot *ḥarīm* 'harem' (voir Vergote 1965, 40 = 98).

C 1. *qūtlu* : *sūḍmu* adjectif substantivé et nom concret (srtt parties du corps).

H. 'ómeq 'profondeur'; 'óšer 'richesse'; 'ózen 'oreille'; 'óref 'nuque';

Ar. 'usr 'difficulté'; šukr 'gratitude'; qubḥ 'laideur, turpitude'; 'udn 'oreille'; lubb 'cœur';

C. mēraš : ég. mūršu 'un blond-roux'; C. wē'ab : ég. wú'bu 'prêtre [un pur]'; c. šēre : ég. šūr(y)u 'fils, enfant [un petit]'; v.-c. ub<sup>7</sup> : ég. (y)úbbu 'cœur'; c. tē'be : ég. ḏúb'u 'doigt'.

F. *qūtlat* : *sūḍmat* ne paraît pas être représenté en arabe.

H. ḥokmāh 'sagesse'; ṭoh<sup>a</sup>rāh 'pureté'; ṭum'āh 'impureté';

C. me'are : ég. mūtrat 'midi [le milieu]'; c. me(e) : ég. mūz'at 'vérité'; c. še'are : ég. šūryat 'fille'.

Les formes c. *hūn* 'intérieur', *kūr* 'personne sourde', dérivés de ég. *ḥúwnu* < *ḥúnwu* et *kúwru*, prouvent que *qūtlu* ne s'est pas changé en *sūḍ* m comme B 1 *qātlu*, B 6 *qītlu*.

B. Schèmes dans lesquels la quantité vocalique du protosémitique s'est modifiée.

B 1. *qātlu* : *sāḏ'm* nom concret et nom verbal.

H. héreg 'assassiner'; ṭēbaḥ 'abattre, égorger'; šéma' 'écouter'; kéleḥ 'chien'; 'éreš 'terre';

Ar. *qatl* 'tuer'; *qaṭf* 'cueillir'; *fahm* 'comprendre', *šarb* 'boire'; *karm* 'vigne'; *šams* 'soleil';

C. *kōtaf* : ég. ḳād'f 'cueillir'; c. *sōtaḥ* : ég. sāṭ'p 'élire'; c. *sō* : ég. sā(w~r) 'boire'.

cōm : ég. kāḏ'm 'vigne, jardin' (exemple unique).

En hébreu comme en égyptien, la chute de la terminaison -u produisit une syllabe doublement fermée et une voyelle épenthétique se développa entre les deux consonnes en contact. En égyptien, la première syllabe, accentuée, qui devenait de ce fait ouverte, exigeait une voyelle longue. Le schème B 6 *qītlu* : *sīd'm* (nom concret) subit le même traitement (noms ségolés de l'hébreu).

B 3. *qátalu* : *sāḏam* nom concret.

H. *zāqān* 'barbe'; *bāšār* 'chair, homme'; *lāḥān* 'lait'; *pārāš* 'cheval';

Ar. *ḏāqan* 'barbe'; *bāšar* 'peau'; *lāban* 'lait'; *fāras* 'cheval';

C. *ōbat* : ég. *zāpad* 'oie'; c. *rōme* : ég. *rāmāṭ* 'homme'; c. *nūte* : ég. *nāṭar* 'dieu';

F. *qátalatu* : *saḏāmat*.

H. <sup>a</sup>*qāmāh* 'terre'; *bērākāh* 'bénédiction'; *ṣ<sup>e</sup>qāqāh* 'justice';

Ar. *šāfaḩa* 'compassion'; *ḩāraka* 'mouvement'; *bāraka* 'bénédiction';

C. *antōre* : ég. *naṩārat* 'déesse'; c. *erōte* : ég. *arāṩat* 'lait'; c. *cōbe* : ég. *gazābat* 'feuille'.

Le même schème, masc. et fém., forme des adjectifs (B 4). Ils sont substantivés en ég. dans la forme féminine, p. ex. c. *amrōše* 'vase d'argile' : ég. \**marāšat* 'une rousse'.

B 5. *qātilu* : *sāḩmu* participe actif.

H. *qōṩēl* 'tuant'; *kōhēn* 'prêtre (servant)'; *zāqēn* 'vieillissant, vieillard';

Ar. *sāmi* 'écoutant'; *kātib* 'écrivain, scribe'; *rākib* 'chevauchant, cavalier';

C. *čoy* : ég. *ḩāzyu* < *ḩāziyu* 'bateau (celui qui fait passer le fleuve)'; c. *wosar* : ég. *wāsrū* < *wāsiru* 'rame (quelqu'un qui rame)'; c. *onəh* : ég. *ānḩu* < *āniḩu* 'cour (quelqu'un qui entoure)'; c. *hobəs* : ég. *ḩābsu* < *ḩābisu* 'couverture [quelqu'un qui recouvre]'.

F. *qātilatu* : *qāṩlat*.

H. *ḩōmāh* 'rempart' (< *ḩāmijāt* 'celle qui protège'); *ōlāh* 'holocauste' (< *ālījat* 'celle qui monte');

Ar. *kāṩiba* 'écrivain';

C. *rompe* : ég. *rānpāt* < *rānipāt* 'année (ce[lle] qui se rajeunit)'; c. *mo'ne* : ég. *mān'e* < *māni'at* 'nourrice (celle qui allaite)'.

Ce schème est toujours substantivé en égyptien. La forme qu'il prend en égyptien est déterminée par les facteurs 2 et 1 (cf. p. 194). La présence du *i* originel est garantie, p. ex., par le pluriel de c. *čoy* : *ečēw* < *ḩaziwu* < *ḩāziyu*. Le féminin conserve l'accent sur la première syllabe à cause du *ā* protosémitique (cf. au contraire A 10, B 3 etc.).

Sur la base de données comme celles qui précèdent, on arrive au tableau suivant des correspondances entre l'égyptien et :

I. l'hébreu et l'arabe :

a. à part égale

A 2 *qittilu* : *siḩḩimu* nom d'agent

B 2 *qatālu* : *saḩām* nom verbal et nom concret

A 4 *qatūlu* : *saḩūmu* adjectif et part.

B 3 *qāṩalu* : *sāḩām* nom concret

A 8 *qīlu* : *sim* nom concret

*qāṩalatu* : *saḩāmat*

*qīlatu* : *simāt*

B 5 *qātilu* : *sāḩmu* participe actif

A 10 *qālqalu* : *sāmsam* nom concret

*qāṩilatu* : *sāḩmat*

*qālqalatu* : *samsāmat*

B 7 *qīṩalu* : *siḩmu* nom concret

A 11 *qīlqīlu* : *simsim* nom concret

B 8 *qatīlu* : *saḩīm* adjectif et participe

*qīlqīlatu* : *simsimāt*

*qāṩilatu* : *saḩīmat*

A 14 noms masc. à 4 radicales

B 10 *qātulu* : *sāḩum* nom concret

b. le féminin fait défaut en arabe ou il y est douteux; l'arabe n'a pas de '*Sprossvokal*'

- |                                                                                |                                                                                        |
|--------------------------------------------------------------------------------|----------------------------------------------------------------------------------------|
| A 1 <i>qattālu</i> : <i>saḏḏāmu</i> nom d'agent                                | B 1 <i>qātlu</i> : <i>sāḏ~m</i> nom concret, nom verbal                                |
| A 3 <i>qatūlatu</i> : <i>saḏūmat</i> n. abstr. qualité, action                 | B 4 <i>qātalū</i> : <i>sāḏam</i> adjectif<br><i>qātalatu</i> : <i>saḏāmat</i>          |
| A 5 <i>qītlu</i> : <i>sīḏmu</i> nom d'action<br><i>qītlatu</i> : <i>sīḏmat</i> | B 6 <i>qītlu</i> : <i>sīḏ~m</i> nom concret<br><i>qītlatu</i> : <i>sīḏmat</i>          |
| A 6 <i>qālu</i> : <i>sam</i> nom concret<br><i>qālatu</i> : <i>sāmat</i>       | C 1 <i>qūtlu</i> : <i>sūḏmu</i> adjectif substantivé<br><i>qūtlatu</i> : <i>sūḏmat</i> |
| A 7 <i>qālu</i> : <i>sāmu</i> nom concret<br><i>qālatu</i> : <i>sāmat</i>      |                                                                                        |

## II. l'hébreu seul :

- |                                                                                   |                                                       |
|-----------------------------------------------------------------------------------|-------------------------------------------------------|
| A 9 <i>qīlu</i> : <i>sīmu</i> nom du grand nombre<br><i>qīlatu</i> : <i>sīmat</i> | B 9 <i>qatīlu</i> : <i>saḏīmu</i> nom du grand nombre |
|-----------------------------------------------------------------------------------|-------------------------------------------------------|

## III. ni l'hébreu ni l'arabe :

- |                                                |                                                               |
|------------------------------------------------|---------------------------------------------------------------|
| A 10 <i>qālqalu</i> : <i>sāmsam</i> verbe      | B 6 <i>qītlu</i> : <i>qīt~l</i> verbe                         |
| A 11 <i>qīlqīlu</i> : <i>sīmsim</i> verbe      | C 2 <i>qa/utūltulu</i> : <i>sa/udūmdum</i> verbe; nom concret |
| A 12 <i>qatāltalu</i> : <i>saḏāmḏam</i> verbe  |                                                               |
| A 13 <i>qatīltīlu</i> : <i>saḏīmḏim</i> verbe. |                                                               |

Il résulte des règles énoncées à la p. 194 ainsi que des parties Ib et II du tableau que l'égyptien a avec l'hébreu plusieurs points de plus en commun qu'avec l'arabe. Cela n'indique cependant pas, à notre avis, une parenté plus étroite; la distribution quelque peu différente de certains schèmes peut être l'effet du hasard. Quant aux phénomènes pour lesquels l'égyptien occupe une position isolée (subdivision III), il est à noter que les schèmes *qatāltalu*, *qatīltīlu* et *qa/utūltulu* servent en tigré et en tigrigna à créer des noms d'action, donc apparentés aux infinitifs de l'égyptien. D'autre part, G. Lefebvre a aussi observé que le berbère forme des thèmes verbaux à signification intensive par reduplication de la racine tout entière (cf. *qālqalu*, *qīlqīlu*) et par reduplication de plusieurs des radicales (1955, § 4,30). Nous ne croyons pas qu'il faut en conclure à une influence, dans ce domaine, des langues chamitiques. Dans une langue où la structure des sémantèmes est essentiellement fondée sur les changements internes, l'emploi des racines entièrement ou partiellement redoublées afin d'exprimer l'action intensive et afin de former des onomatopées est un procédé tout naturel et il peut parfaitement relever de l'évolution interne.

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## DISCUSSION

HODGE: Just one brief statement regarding Professor Vergote's paper. This is a situation in which it is possible to do what he has done and within the rest of the field it is, you might say, absolutely impossible because it takes the early records of Egypt to make such a reconstruction feasible and we do not have the records that, say, would enable us to do this in Chadic. I do not know that we will ever be able to do what he has done in these other areas but I think that what he has done is extremely important for the internal relationships—you might say the dialectology—of Hamito-Semitic.





## EGYPTIAN AND THE OTHER HAMITO-SEMITIC LANGUAGES

WERNER VYCICHL

Why Egyptian? There are several reasons why Egyptian has been chosen as a starting point for the study of the different Hamito-Semitic languages. This choice was due to Egypt's almost central position within the extensive Hamito-Semitic area, because of the great age of its written documents which go back to the third millennium B.C. and also because this idiom, preserved in a vowelless script through many centuries, seems to deserve particular attention.

### CLASSIFICATION OF THE HAMITO-SEMITIC LANGUAGES

This linguistic group may be divided into two main branches, Hamitic and Semitic.

HAMITIC	SEMITIC
Egyptian, and its daughter language	Akkadian
Coptic	Canaanite (Hebrew, Phoenician, etc.) together with Ugaritic
Berber	Aramaic
Cushitic	Arabic
Chado-Hamitic	South Arabian, with Ethiopic
	the Mahra group

Meroitic, now extinct, is listed among the Hamitic languages by Tucker and Bryan (1956), 153, but there is NO EVIDENCE WHATSOEVER of its appurtenance to this group.

The term 'Chado-Hamitic' was coined by Prof. J. Lukas, who first recognized the existence of this Sudanese language group. It therefore seems PREFERABLE to 'Chadic'.

The subdivisions of Semitic are those given by Murtonen (1967).

## HAMITIC AND SEMITIC

If 'Semitic' is a generally accepted term, 'Hamitic' is not. Some authors prefer to see four distinct groups in Africa (Egyptian, Berber, Cushitic, Chado-Hamitic) and one in Asia (Semitic). There is, however, at least one highly important feature common to all four African groups, so that the denomination 'Hamitic' may be regarded as justified. The main difference between Hamitic and Semitic languages is in their verbal system: Hamitic languages have biconsonantal roots, Semitic have not. It is obvious that Hamitic represents an older state than Semitic.

Egyptian: *wn* 'to open', emphatic *y-wn* (\**iwwān*)

Berber: *gën* 'to sleep', durative *ëggan*

Beja: *dif* 'to go', \**ddif* (sg. *ndif*, pl. *edif*)

Hausa: *gina* 'to build', (durative form lost).

A participle of the emphatic form of Egyptian *qd* 'to build' is *y-qd-w* 'mason', Coptic *ekōt* (old form \**iqqādaw*). A similar form is Coptic *ešōt* 'merchant' (old \**iššādaw*; comp. South Arabian *šyṭ* 'to trade'). In Beja, gemination has been replaced by a nasal group (*nd*, for *dd*) in the singular and compensated by vowel lengthening in the plural (O. Rössler).

In these cases the durative (emphatic, present) form shows gemination of the first consonant. Chado-Hamitic has lost its genuine durative form.

## ETYMOLOGIES

In order to reduce personal views on the reliability of etymologies, a classification based on purely objective criteria has been elaborated to make a clear distinction between equations acceptable without discussion and others with a more or less high degree of probability.

Each etymology is classified by means of two figures, the first one concerning the meaning, the second one the phonetics. In both cases the highest figure is 3 and the lowest 0. An etymology with an index of 33 may be considered as certain (e.g. Egyptian *ḥsb* 'to calculate': Arabic *ḥsb* 'to calculate'); intermediate indexes, such as 02, 13, 23, show different degrees of probability and 00 means that the etymology has to be discarded. The values of the figures are distributed as follows:

## (a) meaning

3: identical meaning in both languages,

2: slight difference in meaning, a similar change being found in the same language group,

- 1: different meaning with the possibility of a change, this being attested in more remote languages,
- 0: change of meaning not attested in any existing language.

## (b) phonetics

- 3: all phonetic elements (= radicals) agree,
- 2: one irregularity (correspondence of sound, metathesis, etc.),
- 1: two irregularities,
- 0: three irregularities.

It goes without saying that all available forms of an etymon should be taken into consideration. Thus Kabyle *ar* 'lion' and Hebrew *'ari* 'lion' would seem to be a good etymology. As a matter of fact, however, the Berber word is *ahar* in Tuareg (pl. *iharren*, with a root *harr*) and *aḥur* at Ghadames while, on the other hand, Hebrew *'ari* or *'aryē* 'lion' corresponds to Ge'ez *arwē* 'wild animal'. So we have the radicals *ḥrr* in Berber and *'rw* in Semitic. The etymology is indexed 31, as there is only one common consonant in the two words, and it is thus not acceptable.

A well known etymology is Semitic *'abn* 'stone': Egyptian *ynr* 'stone'. Here, we have not less than *three* discrepancies: ' : *y*, *b* : *n*, *n* : *r*. In this case, however, the sound changes seem to correspond to certain rules:

- ' : *y* as in *yr.t* 'eye' (lit. 'sight'), Sem. *r'y* 'to see' (in modern Arabic dialects *'arā* (root *'ry*);
- b* : *n* the group *bn* became first *mn*, then dissimilation took place > *mr* [cf. Egyptian *\*yubray* = Arabic *lubnā* 'styrax' (written *lubnay*), also Spanish *hom(b)re* 'man', *hem(b)ra* 'woman' for *hom(i)ne(m)*, *fem(i)na(m)*], then assimilation *mr* > *nr* with elimination of the labial element.

Coptic (*y*)*ōne* goes back to *\*yanr* (same nominal form as Semitic *'abn* 'stone').

These matters have already been discussed by me at length (Vycichl 1958a, 367-405; repeated in 1958b, 70-74). This system is not mentioned in the *Annual Egyptological Bibliography*.

## SEQUENCE DATES

A relative chronology, independent of casual spellings, has been established by myself for Egyptian (Vycichl, forthcoming). It is based on the reduction of the vocalic endings:

- (1) *-ī*: ending of the first person singular (also *\*-yi*);
- (2) *-ū*: plural ending of the noun (vocalic *-ū*);
- (3) *-īy*: ending of the nisba adjectives followed by a vowel, probably *-u*;
- (4) *-ay*: part of words as *\*masday* 'to hate', Coptic *moste*;
- (5) *-āy*: dual ending, perhaps *-āyi*;
- (6) *-at*: feminine ending, probably followed by a final vowel;
- (7) the ending *-w* as in *nbw* 'lord', probably *\*-aw*;
- (8) the plural of the nisba adjectives (*\*-īwū* instead of *\*-īyū*).

In Coptic, we have *-ēw* and *-ayw* (B *amayw* 'seas', where the Semitic genitive *-i* was assimilated to the nisba ending). Bohairic *pistayw* 'ninety' is a plural like Arabic *tis'in*.

The reductions seem to have taken place as follows:

sequence date:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
0	<i>ī</i>	<i>ū</i>	<i>īy</i>	<i>āy-</i>	<i>ay-</i>	<i>at-</i>	<i>aw</i>	<i>-īwū</i>
1	<i>i</i>	<i>u</i>	<i>i(y)</i>	<i>ay</i>	<i>ay</i>	<i>at</i>	<i>o</i>	<i>-īwu</i>
2	—	—	<i>i</i>	<i>a</i>	<i>a</i>	<i>a</i>	—	<i>-ēw</i>
3	—	—	—	<i>e</i>	<i>e</i>	<i>e</i>	—	<i>-ēw</i>

As a rule the reductions are linear, but *-īy-* (3) has disappeared in spite of its long *ī*, while *-ay* (*masday*:- *moste*) has survived thanks to the open vowel.

#### A LOST NOMINAL ENDING

Neo-Egyptian *h'.t-y-w* means: (a) 'hearts', Coptic *hete* (A), and  
(b) 'their heart', Coptic *htēw*.

The form (a) consists of three elements: *h'₃.t*, the nisba ending *-īy* and the plural ending *-ū*: *\*eh'ēt-y-u*. The form (b) consists of four elements: *h'₃.t*, the nisba ending *-īy*, a nominal ending (probably *-u*) and the plural ending *-ū*; *h'.t-īy-u-ū* = Coptic *htēw* (see Vycichl 1959a, 57).

The nominal ending was probably *-u*, as we have *-w* after vowels (*nbw* 'lord': *\*nībaw*, *r'w* 'sun': *\*rī'aw*; Coptic *nēb*, *rē* (B)).

The plural ending (*-ū*) was identical with the ending of the third person (plural) as is the case in some African languages (e.g. in Ewhe, a Sudanic language, where *wo* = 'they' occurs as verbal prefix and as plural ending).

#### EGYPTIAN AND SEMITIC VOWELS

Short *i* in Semitic corresponds to long *ī* in Coptic in open syllable; e.g. Arabic *zift* 'pitch', Coptic *sife*.

Long Semitic *ī* corresponds to Coptic *ē*, as in the ending of the nisba forms: Arabic *-īy* and Coptic *htē-f* (*h'*.*t-y-f*).

Short Semitic *u* corresponds to Coptic *ē* in open syllable; e.g. Akkadian *-kunu* 'you' (acc.) and 'yours' = Coptic *-thēnu* (B). There must have been a sound like French *eu* or German *ō* in Neo-Egyptian.

Long Semitic *ū* corresponds to Coptic *ī*; e.g. Egyptian *\*hayūm-a.t* 'wife' (cf. Arabic *ḥalūb* 'chamelle qui donne du lait') is in Coptic *hīme* < *\*h(y)ūmet*, plural *hyome*. There must have been a sound like French *u* or German *ü* in Neo-Egyptian.

A similar case is *\*qayūd-a.t* 'kite (both a measure and a money unit)' in Egyptian, Coptic *kite*, plural *ekyati* (dialect form).

K. Sethe would have reconstructed *\*ehyómwet*, *\*ekyódwet*.

A full list of Egypto-Semitic vowel correspondences will be given in my "Vocalisation de la langue égyptienne" (Vycichl, forthcoming).

#### THE PLURAL ENDING

I have tried to show elsewhere (Vycichl 1955, 261-270) that there was no plural ending *-w* in Egyptian, only vocalic *-ū*. This view is supported by various arguments:

- (1) the plural ending is not written literally (*-w*) in old texts (Faulkner 1929);
- (2) *h'-w-k* 'yourself' (lit. 'your limbs') is in Coptic *hōōk*. This form cannot go back to *\*ha'wēk* or *\*eh'ōwēk*, but only to *\*hā'ū-ka*, almost like *\*sa'ha'u-ka* 'to place thee' = Coptic *sahōōk*. For the meaning, compare Beja *ē-biyē-k* 'thy limbs' = 'yourself';
- (3) there is no Hamito-Semitic plural ending *-w* (or similar), but we have an ending *-ū* in several languages:
  - (a) Semitic *-ū*: Arabic *-ū* as verbal ending (*qatal-ū*, *yaqtul-ū-na*) and as nominal ending (*-ū-na* nom. from *\*-ū-u-na* and *ī-na* gen. and acc. from *\*-ū-i-na*);
  - (b) Somali *-u*: demonstrative pronouns: *kan* 'this' (m.), pl. *k-u-an*; *kas* 'that' (m.), pl. *k-u-as*, etc.;
  - (c) Hausa *-ū*: *bindiga* 'rifle', pl. *bindig-ū*, etc.

#### THE THIRD RADICAL

It is a well known fact that Semitic—and even Hamitic—roots of similar meaning often share the first and second radical as a basic element from which triconsonantal

roots are derived (e.g. Egyptian *qbb*, *qbḥ* 'to be cool').<sup>1</sup> Hitherto, investigations have concentrated on the first biconsonantal group and a great number of such groups have been identified the meaning of which is beyond doubt. The unknown factor, viz. the third radical, has never been the object of an exhaustive investigation. We can compare it in some ways to the prefixes of Greek, Latin or Anglo-Saxon words in English. One should therefore expect to find a particular *nuance* of all (or some) roots the third radical of which is, for example, *-q*. All roots ending in *-q* need not necessarily have the same origin, however (cf. *amount* and *apathy*, where the *a-* is of different origin).

On the other hand, even roots having widely divergent meanings may go back to the same biconsonantal group, as *conceive* and *deceive*, *dissolve* and *resolve*, *compare* and *prepare*. These examples do not imply any suggestion as to the origin of the third radical. They only aim to show that the possibilities are far more extensive than one might expect at first sight and that word composition, which occurs only rarely in Semitic, must have played an important role in pre-Semitic times.

#### WORD ORDER

There is a striking discrepancy between Semitic (and Egyptian) and Indo-European languages as to the position of the *differentia specifica* in nominal compounds. While Semitic languages place it, as a genitive, AFTER the word on which it depends (*Rekhobh Allenby*), English applies the converse word order (*Allenby Road*). In English, there are many compounds the first element of which may be regarded as a genitive, e.g. *tea-pot* and *coffee-pot*. In these cases the old Indo-European languages used the stem only, as in Greek *dēmo-kratía*.

In Semitic the situation is different. The second element there is not a stem but a genitive, and modern Arabic *finjān qahwa* 'cup of coffee' corresponds in the classical language to *finjān-u qahwat-in* '(the) cup of coffee'. In Egyptian, however, there is

<sup>1</sup> Arabic: *nft* 'to blow on, bewitch'

*nfh* 'to spread (smell), blow (wind)'

*nfḫ* 'to blow'

*nfṣ* 'to blow up, inflate'

*nfs* 'soul, breath'

*nfg* 'to blow (strongly)'

'*qš* 'to bend (wood)'

'*qf* 'to bend, fold, curb'

'*ql* 'to tie up, bind'

'*qd* 'to knot, tie'

*kmy* 'to hide'

*kmh* 'to be blind'

*kmn* 'to be hidden'

*kmr* 'to cover'

*kmm* 'to cover'

*km* 'truffle'.

no trace of a genitive ending and direct genitives are formed merely by juxtaposition, e.g. *Pr Mnṯw* '(the) House of (the) God Month'. If Month (vocalized \**Mančaw*) had had a genitive ending (e.g. *-i*), we should have had some indication of it in the spelling (e.g. \**Mnty* for *Manṯa-i*, or similar).

This word order, '*regens rectum*', of the Semitic languages cannot be the oldest grammatical structure. The same applies to the Hamitic languages. All the nominal suffixes, such as the endings of the feminine gender (*-t*), the dual (*-āy*, or similar), the plural (*-ū* or *-ān*), go back to 'full' words (as Chinese grammarians would say).

It is not intended to discuss here the various aspects and implications of these two fundamental ways of thinking, but there is a considerable literature on this subject, from Wundt and others to my teachers Wilhelm Schmidt, Viktor Christian and Karl Bühler.

#### ORIGIN OF GRAMMATICAL ELEMENTS

Wherever we can trace back the meaning of a nominal ending in our languages, we always find that it was originally a 'full' word. The ending *-ly* in *daily* and *weekly* has exactly the same origin as *like* (Old Norse *likr*, Dutch *ge-lijk*, German *g-leich*). It means 'the body' as in German *Leiche* 'corpse', even 'the living body' (cf. *Leichdorn* 'corn (anat.)'). *Göttlich* 'divine' and *menschlich* 'human' must have meant, originally, 'having the body of a god' and 'having the body of a man'. It is obvious that only a living being or a statue could have been, at the outset, 'göttlich' or 'menschlich', but never a *word*, a *thought*, or a *thing*. But when the primitive meaning faded away, the term could apply to anything having some relation to gods or men.

#### APPARENT MICRO-ELEMENTS

We may assume that all the nominal (and other) endings of Semitic, as well as those of other languages, go back to 'full' words. We shall probably never know the precise meaning of these endings, but comparison with other languages may give us some idea of their original sense. In Bengali, the plural may be formed by adding words like 'group', 'tribe', or 'cast'. Also Egyptian *-ū* or English *-s* (= Indo-European *-s*) must have meant something similar. In Ewe, the particle of the indirect genitive (for alienable objects) goes back to a Sudanic word for 'place'. In Malay and other languages, diminutives are formed by adding the word *anak* 'child'. The dual ending may have meant 'a pair' or 'a couple'.

It would often have been quite impossible to predict the way in which the terms for 'male' and 'female', 'masculine' and 'feminine', were coined in the different languages. In Mongolian, *būsetei* 'with a girdle' means 'male' and *būsegü* 'without girdle' means 'female', for in Mongolia men wear girdles and women do not.

## SYNTAX OF COMPOUNDS

Most fortunately it is not necessary to know the exact original meanings of these elements. It will suffice if we content ourselves with such approximations as: *-a.t* (fem.) 'wife', *-āy* (dual) 'pair', *-iy* (nisba) 'child', *-ū* (pl.) 'group', *-ān* (dim.) 'child', etc. In the case of Arabic *malik-a.t* 'queen' it is obvious that this expression cannot have meant according to the prevailing Semitic syntax 'king of the wife' but only 'wife of the king'. Egyptian *rmṯ-w* 'men' (phon. *rmṯ-ū*) is not a 'man of a group' but 'a group of men'.

All these Semitic and Hamitic formations (feminine, plural, etc.) are built, not according to the rules of Semitic grammar, but on a heritage of a long bygone period. If we call both Hamitic and Semitic the Noahitic group, the previous stage could be called the Lamekhitic one, after Lamekh, Noah's father.

Judging by the grammatical (*-a.t*, *-ū*, etc.) and root-defining (*-q*, *-l*, *-f*, etc.) elements, it would seem that Lamekhitic consisted to a large extent of rather small units unlike the bulk of Semitic words. In this respect we must, however, bear in mind that words used as grammatical elements in all languages show a tendency to become shortened. Another feature of Lamekhitic was its clear and logical structure, both morphological and grammatical, in the sense that the grammatical elements had kept their pristine meaning.

Another question is whether Lamekhitic had already developed the 'unstable' vocalism of Semitic and this we may deny. Semitic vocalism, in the sense that vowels belong rather to the morphological and grammatical (and not so much to the individual) sphere of the vocabulary, is certainly a recent feature due to the generalization of word patterns. There must have been a general tendency to create analogical forms and there appear to exist still visible correlations between certain nominal and verbal patterns. It is, however, not possible in the present state of our knowledge to give precise rules but there is, among others, a certain relation between *a*- and *i*-vowels, both long and short:

perfect:	<i>salim-a</i>	imperfect:	<i>ya-slam-u</i>
adjective:	<i>salīm-un</i>	noun:	<i>salām-un</i>
noun:	<i>silīm-un</i>		
imperative II:	<i>sallim</i>		

The adjective *salīm-un* has been borrowed by other verbal classes as a passive participle (Arabic *qatīl-un* 'murdered'). This must have taken place in Noahitic times as we find it already in Egyptian, e.g. Coptic *ušam* 'dough' from *\*wašīm* (comp. *wōšm* 'to knead'), *\*ha'iq* 'shaven', Coptic *hak* 'bald' (modern Arabic *ḥaliq*) etc. (see Vycichl 1959b, 253-259).

But *qatīl* is not the sole passive participle in Egyptian. There are even cases of *qatūl*-forms (as *ḥayūm-a.t* from *\*ḥalūm-a.t*). In the Berber languages, the normal



form is (a)*mēqtul*, i.e. Arabic *maqṭūl* with the prefixed article (Shilha *anēkruf* 'prisoner', lit. 'the tied one' from *krēf* 'to tie', like Hebrew *'āsir* or Egyptian *sqr* 'nx 'smashed and tied'). A list of Berber participles is given in Vycichl 1969.

## COMPARATIVE PHONETICS

The following synopsis aims at showing how complex the situation of Egyptian is. In the light of comparative linguistics we discover for only six etymologies a relatively large number of dialect forms, contrasting with the usual picture of a uniform language.

As a rule Semitic *q* corresponds to *q* in Egyptian. There are, however, some cases in which *q* is represented by *j* (traditional transcription *ǧ*) and *x* (H. Goedicke).

BERBER	EGYPTIAN	SEMITIC
	'nx 'sandal strap'	'rq <sup>2</sup> 'strap (of sandal)'
	'rq, 'nx 'to swear'	
	<i>dnd, qnd</i> 'to be furious'	<i>qnṭ</i>
	<i>qnd</i> 'ape'	<i>qird</i> 'ape'
	<i>nqdd</i> 'to sleep'	<i>rqd</i> 'to lie down'
<i>ēqqēd</i> 'to cauterize'	(missing)	<i>wqd</i> 'to burn'
<i>iwriḡ</i> (verb), <i>awraḡ</i> 'yellow'	<i>wʒ x, wʒ ǧ</i> (Goedicke), <i>yʒ q.t</i> 'leek'	<i>wraq</i> 'to be green'

## CONCLUSION

Thanks to its geographical position, Egyptian occupies an important place within the Hamito-Semitic area. Further progress in Egyptian linguistics will depend to a large extent on comparative studies with both the Hamitic and Semitic languages.

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<sup>2</sup> Both 'nx and 'rq 'to swear' have a common origin (lit. 'to tie (one's self)'); cf. Arabic *hlf*, Aramaic *'arqēfā di msānā* 'sandal strap'.

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#### DISCUSSION

LESLAU: A slight note of caution without entering into too many details: On p. 203, if the word for 'lion'—Kabyle *ar*, Hebrew *'arī*—is compared with a root *b-r-r-*, a comparison in which as Dr. Vycichl admits there is only one common element, my guess is that we can compare possibly Hamito-Semitic with American Indian languages. Somewhere the line has to be drawn in comparisons, otherwise we cannot compare reasonably any more. Problem no. 2, perhaps of less importance—I am not yet personally convinced that Semitic has an absolutely tri-radical system. I have doubts whether the so-called bi-radical verbs were really originally tri-radical. There is a possibility of a bi-radical system in Semitic too.

VYCICHL: I think only etymologies having an index of 33 should be accepted. If an etymology like that of the word for 'lion' has the index 31 it means that it has to be excluded unless somebody can prove that the wanting radicals can correspond. This is my opinion. About tri-radical roots in Semitic: I think that in my article in the *Acts of the Paris Congress* I have added a list of so-called bi-radical nouns and compared them with Berber and Egyptian words where the third radical is found. For instance in the word for 'blood' the Berber form is a plural, *idammen*, which shows that there was a third radical, probably *w* as in Semitic. It is significant that only nouns are treated as bi-radicals in Semitic and never verbs. I know there is a recent tendency to consider also *mediae infirmae* as bi-radical, but not in the present state of the language, only in a previous stage. But this would agree with my theory that bi-radical roots were found in an earlier stage of the language.

BARR: Could we just get clear about the point that Professor Leslau raised. Are you saying that the etymology of 'lion' is NOT a good etymology?

VYCICHL: No, it is not. It does not have an index of 33.

ULLENDORFF: One of the points made by Dr. Vycichl and already referred to by Professor Leslau is the question of tri-literality. Now if I understand Dr. Vycichl correctly—and he will correct me if I am wrong—he says categorically that there was a tri-consonantal system in Semitic and a bi-consonantal system in Hamitic languages. Am I correct?

VYCICHL: Yes, bi-, tri- and quadri-consonantal in Hamitic.

ULLENDORFF: But there are a large number of bi-consonantal verbal roots also in Semitic languages, not to speak of the so-called hollow roots. But if you consider the extremely large number of verbs in Semitic languages which, against the conventions of Semitic, have identical second and third radicals: in this case surely it is completely clear, and we know from earlier formations that they were formed to correspond with the *Systemzwang*; in other words, they had a third radical by analogy. So therefore the presumption would rather point in the other direction—although I would not wish to make any such statement; but the presumption would point to the direction that Semitic, too, originally had a very large number at least of bi-consonantal roots.

VYCICHL: You will find at the end of my article some examples of Arabic bi-consonantal stems enlarged by a third radical.

ALLEN: I am not really sure whether the mention of the fact that word order in Semitic is the opposite of that of Indo-European really contributes very much. It is essentially the same type of word order that is found in many other languages which are not related to Semitic languages. It is typologically useful but I am not sure that it is generally useful for more than this.

PARSONS: On this question of word order, I might just point out something that has never been explained in Hausa. In three contexts only—in the Hausa words for ‘sunrise’, ‘midday’ and ‘sunset’—you can have the elements in either order, in complete free variation, and with no connecting morpheme at all. I just mention this because it is one of these odd little facts that gets passed over in these comparative analyses.

TYLOCH: Only a point concerning terminology. I think the term ‘Hamitic’ is *passé* since, as Professor Cohen and other scholars pointed out in the fifties, there is no single linguistic unit that could form a counterpart to Semitic. Probably Dr. Vycichl understands by ‘Hamitic’ all the non-Semitic languages of the family.

VYCICHL: If we have a complex like *malikat* ‘queen’ it is obvious that the primitive word order was, as in Indo-European, ‘king wife’ or ‘king’s wife’—with elements

having grammatical function being derived from originally full words. According to Semitic grammar it would mean *malik* 'the king' plus 'of a wife', which does not correspond to the meaning. This example and others leads us to the conclusion that Hamito-Semitic languages had a different word order from that of the Semitic languages today. Similarly with the personal affixes—'my house' is constructed as 'house of me' and here the genitive is postfixed. Languages which have developed freely, like Coptic, have developed these into prefix patterns.

PETRÁČEK: There is a study by Horwitz on the third radical of the Semitic root, published about thirty years ago if I am not mistaken. The Semitic root is surely not merely the three radicals, to it belong also the relations of incompatibility. Thus I have studied incompatibility in Egyptian in the back series, that is to say the pharyngals etc. The situation proved quite different: in terms of these internal relations it is a pre-Semitic root structure and not a Semitic one.

## **IV**

### **LIBYCO-BERBER SECTION**



## THE RECONSTRUCTION OF PROTO-BERBER SHORT VOWELS

KARL-G. PRASSE

### INTRODUCTION

It is a well known fact that quantitative oppositions are operative in the vowel systems of the Semitic languages as well as in Egyptian and Cushitic (and Chado-Hamitic, if we choose to extend the analysis so far). Semitic has a system of three vowel qualities, *a*, *i*, *u*, each of which may be either short or long, the phonemic value of this quantitative opposition being clearly attributable to proto-Semitic. It appears probable, however, from internal evidence, that the proto-Semitic quantity distinctions are a secondary development and that pre-Semitic must have known a stage with one vowel length only (Fleisch 1968, 59-61 and passim). If it could be shown that the proto-type of at least one other of the branches of Hamito-Semitic also distinguished two vowel quantities in a similar way to proto-Semitic, then the said stage of non-distinction would even have to be assigned to pre-Hamito-Semitic.

Egypto-Coptic appears to have had vowels which were long by position (in open accented syllables; Edel 1955-1964, §§ 153-157, 267) back to the period of the Old Kingdom. Cushitic can be shown to have phonemically relevant oppositions of vowel length. To my knowledge, however, it has not yet been possible to decide whether these oppositions are secondary developments or original (Tucker and Bryan 1966, 495 ff.; e.g. 508, plural by shortening).

This state of affairs indicates that vowel quantities may have been already differentiated in common Hamito-Semitic. Consequently it becomes imperative to ask the question whether Berber has, or can be shown to have had, pertinent oppositions of vowel quantity.

### VOWEL SYSTEMS IN MODERN BERBER

Contrary to what has been hitherto more or less tacitly assumed, I feel convinced that Berber does not have a vowel system which is roughly identical throughout all the dialects. I believe that there are at least three or four systems, differing from

one another by the number of vowel phonemes. Since the geographical limits of each system are not yet known, we will simply choose as cardinal types four well-known dialects.

#### THE TASHELHIT SYSTEM: (ə), a, i, u

The simplest vowel system is the one known to be operative in Tashelhit (= Fr. chleuh), the dialect-group of South West Morocco, which simply opposes three vowel qualities. It is true that differences of quantity have been noted, but these appear to have no phonemic value and to depend rather on syllable structure and accentuation, with open accented syllables having the longest variants.

In addition there is an auxiliary vowel ə, which has no phonemic value but is simply inserted at certain places to facilitate the pronunciation of consonant clusters.

This is the vowel system regarded by André Basset as typically Berber and which induced him to name the vowel ə 'voyelle zéro'. Galand has shown how it is possible to write Tashelhit unambiguously without employing the auxiliary vowel at all.

The Tashelhit system appears to be operative also in Tamazight (Central Morocco), and perhaps also in the Rif dialects (Northern Morocco).<sup>1</sup>

#### THE KABYLE SYSTEM: (ə), a, i, u

The Kabyle dialects of Algeria seem to have a system almost identical with that of Tashelhit.

However, the use of the auxiliary vowel seems to be governed by more rigid rules in Kabyle than in Tashelhit. For each accentual unit only one syllable structure seems in fact to be possible; thus *əkrəs* 'tie!' becomes, with suffix pronoun, *kərs-it* 'tie it!' and similarly we have the forms with personal affixes *təkrəs* 'she tied', *kərsən* 'they tied'; etc., *yəkrəs* 'he tied' 3.m.sg., with suffix pronoun, *ikərs-it* 'he tied it'. The structure required is determined by counting backwards from the end (Vincennes and Dallet 1960, 34).

A form like *tkərs* for *təkrəs* appears to be irregular even if in context there should happen to be a vowel before and after it (e.g. *ulammas təkrəs anyir-is* 'even if she knit her brow').

This does not mean that I contest the validity of such descriptions as A. Basset's "ə, allant, suivant la rapidité du discours en particulier, de la voyelle bien marquée jusqu'à l'évanouissement total" and "nombre et position des ə varient aussi parfois en raison de la nature des consonnes" (Basset et Picard 1948, 9, 196), but I doubt whether these observations normally receive a correct interpretation. I feel that the native Kabyle, when taught to write, is not normally in doubt as to the existence or

<sup>1</sup> Professor L. Galand of Paris states in a recent letter to me that he shares this opinion.



position of any *ə* vowel; careful pronunciation automatically reveals them to him. The fact that ambiguity in this respect depends on the speed of speech has not, to my mind, been sufficiently underlined.

It is particularly noteworthy in this connexion that Kabyle seems to maintain as far as possible the consonantal group in verbs of the type *əls* 'to wear' (*yəls* 'he wears') but permits the insertion of a *ə* in those of the type *gər* 'to throw' (*igər* 'he throws'). This means that it is possible, from the mere position of the vowel *ə*, to distinguish between such verbs as *əzəd* 'to weave' and *zəd* 'to grind'.<sup>2</sup>

I suspect that this system of Kabyle is somehow transitional between that of Tashelhit and those systems with one central vowel having an even more stable position and enjoying genuine phonemic status. Such systems, however, have as yet been described too superficially for all uncertainty regarding their nature to have been removed.

*The Nefusi system: ə, a, i, u(?)*

Thus F. Beguinot states that, in the Fassâto dialect of Nefusi, verb forms with personal suffix like *kərsən* and *kərəsən* (< *əkrəsən*?) are equally possible (e.g. *fəhamən* or *fəhmən*).<sup>3</sup>

*The Zenaga system: ə, a, i, u(?)*

A still more stable *ə* has been noted in the poorly known dialect of Zenaga (e.g. *askərəm* 'do!', imp. pl.; *tokchaməm* 'you entered', etc.; R. Basset 1909, I, 26 ff.).

*The Awgila system: ə, ä, a, i, u(?)*

Similar patterns have been noted in the dialect of Awgila (e.g. *ir ahlében* 'so that they might pass'). Particularly interesting are such forms as *alemâd* vb.n. of 'to learn', *aregâz* 'person'. It remains to be investigated whether this dialect distinguishes two central vowels, *ə* and *ä* (Paradisi 1961, 79-91, v.p. 80/10; 1960, 157-177).

Note that, for all these little known dialects, open syllables ending in a -*ə* have been reported as being possible.

<sup>2</sup> L. Galand informs me that, although inclined to accept the possibility of such oppositions in Kabyle, he considers them as FOSSILE and optional and thus external to the operative phonological system of Kabyle. He is convinced that Kabyle *ə* is not an autonomous phoneme, but agrees that Kabyle has a greater number of pronounced *ə*'s than the Moroccan dialects, owing to a simpler and more rigid syllable structure.

<sup>3</sup> Beguinot (1942), 52-53. I am ignoring for the present purpose Beguinot's highly detailed description regarding the phonetic realization of *ə*.

## THE GHADAMSI SYSTEM: ə, ä, a, i, u, e, o

The dialect of Ghadames has been shown in a recent publication (Lanfry 1968) to have a seven vowel system. Because of the recentness of this publication, although not of the discovery itself which dates back to the end of the Second World War, we should perhaps give some examples of these hitherto unknown oppositions: *we* 'it' / *wi* 'these'; *den* 'there' / *din* 'when?'; *yōfo* 'it swelled' / *yūfō* 'he found'; *yōzān* 'he weighed' / *yuzān* 'he sent'.

The imperfect is differentiated from the perfect by means of the oppositions ə/ä or i/e (*tākrəs* 'she ties' / *tākräs* 'she tied'; *tākri* 'she returns' / *tākre* 'she returned').

The position of both central vowels is invariable.

Since the Ghadamsi system contains the same number of vowels as the Twareg system, and as Ghadamsi ə and ä would appear like their Twareg counterparts to be not always of central quality, it is legitimate to wonder whether the two systems are not in reality wholly identical—that is to say, whether Ghadamsi ə and ä are not really short, in contrast to the other five vowels which would then be long.

In fact Lanfry states that ä (his *e*) is "de longueur moyenne ou brève", while he does not expressly define ə. For the other vowels he states that they may be "de longueur moyenne ou longue" and he writes the long variants by means of a stroke placed under the vowel. This seems in fact to point to a system similar to that of Twareg, the (over)long vowels being merely conditioned variants of the long or middle length ones. However, the specimens of Ghadamsi poetry given by Lanfry are not in favour of such a hypothesis:

## SPECIMEN OF GHADAMSI POETRY

Metre: 7 syllables.

*S-älbarkät-n-älmüdu-yo,*  
*Älmüdu m-Baba Rābbi;*  
*S-älbarkät n-älmüdu-yo,*  
*D-imātran-i täddännin!*  
*Yāhlu Rābbi 'ält-Ulid;*  
*Yācmār Rābbi Wazitēn;*  
*Yākkās Rābbi 'älžēl-o,*  
*Älžēl-o dəffār yāššuf".*  
*Iyyamāt a twažatēn,*  
*[z]n-nəwānmāt älmāmbēr,*  
*[z]n-nədeümāt i-Yünās*  
*Yässəbbābän i-Tünās!*  
*Äz[z]iyyäzän əssäbyan,*  
*Äğəllälnāt tiyəndar.*  
*Äkkərmāt a tkarürēn,*  
*N-nəθθärmāt Baba Rābbi,*  
*N-nəθθärmāt Baba Rābbi,*  
*Änäe-d-yābb arābän-i!*  
 (Lanfry 1968, 143-144).

This poem, which appears to be typical of the poetry collected by Lanfry, does not support the hypothesis that *ə* and *ã* are short (that is, form short open syllables) as in Twareg poetry. Ghadamsi would therefore seem to have no opposition of vowel quantity; all verses must, however, have the same number of syllables as in Twareg.

No trace has been discovered of a particular function of the overlong vowels in possible intensive verb forms, although possible sporadic instances of such forms should be noted (thus *äniddämän* for expected *änəddämän*; Lanfry 1968, 22-27).

#### THE TWAREG SYSTEM: SHORT *ə*, *ã*; LONG *a*, *i*, *u*, *e*, *o*

Like that of Ghadamsi, the Twareg system is a seven vowel one but here two vowel quantities can be clearly distinguished. Firstly, under certain conditions (in an initial or a stressed syllable, when in contact with certain consonants), *ã* may have clear front or back articulation (*ǣ*, or by assimilation *ě*). Secondly, in contact with *w* or *y*, *ə* may present itself as a clear *ũ* or *ĩ* respectively. These variants are distinguished from *a*, *e*, *u*, *i* merely by the fact of their being short.

To this it must be added that final *u*, *i*, *o*, *e* have the diphthongized variants *ũw*, *ĩy*, *õw*, *ěy* (or *ǣw*, *ǣy*) before hiatus and, it would appear, before pause. This means that in hiatus, where the final *w* or *y* goes to the following syllable, the *ũ* or *ĩ* forms the centre of a short open syllable.

The poetic metres are built upon these quantitative oppositions, and this is perhaps the best proof of their reality if we reverse the opinion expressed by André Basset: "Pour qu'une métrique soit quantitative, il faut que la langue soit elle-même quantitative et que la structure quantitative de la métrique réponde à la structure quantitative de la langue" (A. Basset 1952, 4-5). As the quantitative structure of Twareg has not yet been generally admitted, and since I cannot today continue to support the metrical theory of Ch. de Foucauld in all its details, it is best that I illustrate my point of view by quoting a specimen of Tahaggart poetry (see below).

The existence of a special vowel *ã* as distinct from *ə* has long been suspected, but it is only quite recently that I have been able to prove, with the aid of a Kəl-Dənnəg informant, that it is always distinct from *ə* in quality except in certain clear cases of assimilation (Prasse 1973).

It should finally be added that Twareg in my opinion also possesses overlong vowels, as already foreseen by Foucauld who notes three vowel quantities. In most cases these overlong vowels are, it is true, mere variants of the long ones in open stressed syllables; but already in the nominal system not all overlong vowels can be so simply defined (e.g. those of the penultimate of the verbal adjectives of type *āmākras* 'who ties'). And far more important — the overlong vowels constitute a morpheme in the intensive verb forms as opposed to the simple ones, and it should be noted that, contrary to the opinion of Foucauld, these vowels are not opposed to short ones in the perfect in all verbs, but to short ones in certain verbs and to long ones in others.

SPECIMEN OF TWAREG POETRY (*tahaggart*)

Metre: *sěyěnin* - - / ˘ - / - ˘ - / - - .

*Hīy Abādal a hīy-in-ya āwnaf*  
*ən-Dīdīy iggārān atāklas;*  
*əsmāḍrānāy tabārat. Nnīy-ās*  
*y-əmis-nānāy: "Sikk-īy, en-nīfsas".*  
 (Foucauld No. 16).

*Hīy Amədid aḡ ḡānāt tkūnīn;*  
*ḡīy enəkir d-əs, ānīkarin,*  
*nəḡla d-təra d-tīy əhosāynīn.*  
*Āqqimān əmyad ākīrarin.*  
*Wa n-Təmmənīt emīy isāṭsin*  
*ihā ehān, əylāynāt-ās trikin:*  
*zərd a tāmūs y-āləs, tiheyīn*  
*y-ara n-tāməṭ[ʔ] akəd i n-thārrīn!*  
*līy sūri, līy ti n-dādān rāsīn.*  
 (Foucauld No. 101).

Only open syllables ending in *ə* or *ā* (*ǎ*, *ǝ*) are short. The syllable limit does not coincide with the word limit (e.g. *Hī-γA-bǎ-da-la-hī-yin-ya āw-naf*. The vowel before hiatus is normally elided (e.g. *inγ(a) āwnaf*, *ih(ā) ehān*).

A syllable of the wrong quantity is tolerated in about 5% of the cases. The vowel quantities postulated thus are the result of a statistic evaluation. It should be particularly noted that the full vowels of certain perfects, written as short by Foucauld, are properly long.

Foucauld claims the metre to be: ˘ - / ˘ - / - ˘ - / - ˘ .

## INTERMEDIATE RÉSUMÉ

To sum up, we have in all Berber dialects a distinction between central vowels and what André Basset calls full vowels ('voyelles pleines') with clearly front or back articulation. Some dialects have only one central vowel and three full vowels. These may require subdivision into those which have a phonemically pertinent *ə* and those which have an auxiliary non-phonemic *ə*; their *ə*, however, is always central except in quite exceptional circumstances. Some other dialects have two central vowels and five full vowels. Their *ā* especially may frequently have full vowel quality (*a*). These dialects have two additional full vowels, *e* and *o*, which appear in all cases to have developed from *i* and *u* respectively but which have manifestly acquired phonemic status. Finally, these latter dialects have a clear or possible opposition of two vowel quantities, the central vowels being short, the full vowels long. We find

in all dialects special overlong vowels, but these seem to be mere variants of the full or long vowels. Only Twareg is possibly a partial exception to this rule, its overlong vowels possessing phonemic value in the intensive verb forms. The problem is whether their length should be regarded as secondary to their accentuation or vice versa.

We now come to the question "What can be gathered from these synchronic data for the reconstruction of proto-Berber vowels?".

#### GENERAL REFLEXIONS ON RECONSTRUCTION

As formerly stated, *e* and *o* of the Twareg/Ghadamsi system can be shown to have developed from *i* and *u* respectively by simple phonetic changes like vowel assimilation etc. Likewise Twareg *ẽ* can be shown to be a variant of *ā* or *ə* in most cases. It has not yet been possible to explain all cases, but it seems by now to be fairly certain that it will eventually be possible to explain them all. One of the most intricate cases is that of the 'state' vowel (*e* in the sg. as opposed to *i* in the pl., both being represented by *i* in Northern Berber). This situation is known also in Ghadamsi. On the basis of our present knowledge, then, it would seem that we can rule out *e*, *o* and *ẽ* as possible proto-Berber vowels.

The situation is quite different in the case of the central vowels *ə* and *ā*. Firstly, it appears to be impossible to find any sound laws which could account for the distinction between the two in Twareg/Ghadamsi as being secondary. We must therefore assume that the difference is original. And since we can show that *ə* and *ā* both correspond to the unique *ə* of Tashelhit/Kabyle, or rather that they do not correspond to the full vowels of these dialects, we must further assume, that Kabyle *ə* is due to the merging of original *ə* and *ā* into one vowel, and that Tashelhit zero represents a further reduction of this unified vowel.

It is true that, theoretically, one could imagine the opposite to have taken place. It is well known that Indo-European at least must have once passed through a stage of having one vowel only and, as this vowel had apparently a variable place just like Tashelhit *ə*, this amounts to saying that at that period Indo-European had no vowel at all. The vowel qualities directly observable to us are the result of the vocalization of sonants, the contraction of the auxiliary vowel with laryngals, and the action of stress (de Saussure 1879). Something similar could of course have obtained for the Berber central vowels. In such a case Tashelhit might be assumed to have the most archaic structure, its zero vowel having the same function as the unique vowel of the above mentioned period of Indo-European. Kabyle *ə*, Twareg/Ghadamsi *ə* and *ā*, would then be further consolidations and differentiations of the former auxiliary vowel.

Now it is equally well known that central vowels can in most cases be shown to be reductions of front or back vowels by reason of their brevity or their unstressed

position. If we are to make a choice on purely statistical grounds, we must prefer the assumption that Berber zero is not original, and that even *a* and *ā* are not so although nearer to the original situation than absolute zero.

If, on the contrary, we were to assume that zero is original, this could only be because it had been proved to be so in detail. No such proof, however, exists and in particular, as I have already said, the difference between *a* and *ā* has not been shown to be secondary. Moreover this hypothesis seems to be contradicted by the first conclusion of the following section, namely that *a* and *ā* occupy the same positions as the full vowels of corresponding weak verbs.

As statistical reasons make it improbable that even *a* and *ā* could be the original qualities of the corresponding proto-Berber vowels, we must now look for a means of determining whether their quality was different in proto-Berber.

We have, in my opinion, one good method at hand, namely the analysis of the secondary full or long vowels, which are due to the contraction of a 'central' vowel with a lost weak consonant for which I use the symbol *\*h*. Those who have followed my work during recent years will know that this is the way I view the vowels which have formerly been described as occupying the place of a radical consonant. Unfortunately space does not allow me to enumerate here the reasons why I do not admit the existence of radical vowels in Berber.

A second question arises in this connexion: "Can some of these secondary vowels not be due to contraction with full or long vowels?" In my opinion some are, but it is easy in most cases to determine when this is the case.

A third question must necessarily be asked: "Are the modern quantitative differences original and are they so in all cases?". My answer is that I find that most modern central vowels cannot be shown to derive from long ones and must therefore be regarded as having been originally short since their shortness seems to be the only reason for their reduced state. However, there exist short vowels which can be shown to derive from originally full or long ones and, vice versa, there exist vowels which are due to lengthening of originally short vowels. The final reason is that in neither case is the phenomenon pan-Berber.

I shall now devote a paragraph to each of these phenomena.

#### SECONDARY FULL OR LONG VOWELS IN BERBER

The method of reconstructing proto-Berber groups of short vowels + *\*h* from modern full or long vowels works on the assumption that in proto-Berber the same patterns were formed from strong as well as from weak roots. This is a method which has also proved fruitful in Semitic. An excellent example is that provided by the simple tenses of the tri-literal verb (my 1st conjugation).

From the table given at the end of this paper the following conclusions can be drawn:

(1) The original positions of the short or central vowels of the strong tri-literal verb stem must be before the first and the third radicals. These are the positions they still regularly occupy in Twareg and Ghadamsi, as well as being those indicated by the corresponding full or long vowels of weak verbs.

(2) Judging from the qualities of the full vowels of the weak verbs, the proto-Berber short vowels would appear to have had three different qualities, *a*, *i*, *u*, like the long ones. The system would thus resemble the proto-Semitic one: \**ā*, \**ī*, \**ū*; *ā*, *ī*, *ū*.

This assumption rests upon the supposition that the lost radical had no influence whatsoever on the quality of the vowel. This would normally be so with a lost laryngal. In fact we have certain archaic dialects which, in certain verbs, preserve an *h* preceded by a central/short vowel, e.g. Twareg *əlku* 'to despise', Taneslemt (region of Timbuktu) *əlkaḥ* (Ghadamsi: *älkäḥ*).<sup>4</sup>

A serious problem as yet unsolved, however, is the *u* (Ghad. *o*) which corresponds in a whole series of Berber dialects to certain *a*'s (the *o* of Ghadamsi *ils*o).<sup>5</sup> The fact that not all *a*'s are subject to this colouring might indicate the influence of a particular lost radical. However, upon closer investigation the problem proves to be more complex. Thus the demonstrative pronoun usually appearing as *wa* is represented in Ghadamsi by *wo*, but in the same dialect we also find a *wa* which one would normally be inclined to believe to be the same word, namely in the interrogative *wa-din* 'which?' and the interrogative particle *wa* '(or) what?'.<sup>6</sup>

This phenomenon is no doubt associated with the fact that, in the dialects which have perfects in *-u*, this *-u* is elidable and the verb requires the form of the direct suffix pronoun with initial *i*(*e*); thus Ghadamsi *ils(o)-et(t)* but *imda-t(t)*. Kabyle has *-a*, not elidable before suffix pronoun, in both types; Twareg has elidable *-a* in both and requires the suffix pronoun with initial *i*(*e*). In either case a levelling seems to have taken place compared with Ghadamsi.

A second problem, this one pan-Berber, is the presence of secondary *i*(*e*) in certain verb forms instead of the expected *a*. Thus the types *äls* and *əmdu* both have *i*(*e*) in the 1st and 2nd persons sg. of the perfect (*əlsiy*, *əmdiy*; *təlsid*, *təmdid*). Certain conjugations have this vowel before all endings.

(3) The vowel *ä* of Twareg/Ghadamsi corresponds to full *a* of the weak verbs and must be supposed to have its origin in a short \**ä*.

(4) The vowel *ə* of Twareg/Ghadamsi corresponds to both *u* and *i* elsewhere and must represent a contamination of short \**ū* and \**ī*. Final *i* < \**ih* seems to be more

<sup>4</sup> Cf. Prasse (1969). It would appear necessary to rule out the possibility of *w* and *y* being involved in such cases, as these behave differently and do not as a general rule undergo contraction either with full/long vowels or with Twareg *ä*.

<sup>5</sup> Cf. Destaing (1919) and (1921). The latter deals with cases of *wu* (*wo*) etc. corresponding to *wa*, etc.

likely to disappear than final  $u < *ǃh$ . It has been preserved, however, before the endings of the pl. in Twareg/Ghadamsi and also in many other (derived) verbal and nominal forms; e.g. Twareg *māsalsi* 'to cover one another, to wear together, to share (a garment)', *asālsi* 'the fact of clothing (caus. inf.)'.

(5) The distinction between the imperfect and perfect tenses of the conjugation illustrated in our table was achieved in proto-Berber by a double vowel opposition throughout, one in the pre-radical vowel and one in the pattern vowel before the last radical (my 'characteristic vowel'):

*\*yākrīs* or *\*yākrūs*/*\*yūkrās*  
*\*ākrīsān* or *\*ākrūsān*/*\*ukrāsān*.

The characteristic vowel is still differentiated in Twareg and Ghadamsi before a strong radical, in spite of the contamination of  $*i$  and  $*ū$  and probable alteration (centralizing) of the quality of  $*ā$ . It varies in a way similar to that of the active (agentive) tri-literal verb in Semitic.

The preradical vowel is only partially differentiated in Twareg and Ghadamsi, namely in the type *āls* and in Ghadamsi also in the type *ākrās*. The differentiation has been lost in the type *alku* ( $< *ālku$  by 'Umlaut' produced by the  $-u?$ ) and in Twareg in the type *əkrəs* ( $< *ākrās$ ). This warns us against automatically deriving every Twareg or Ghadamsi  $ə$  from  $*i/ū$ ; we cannot reach a decision until after comparison with other types showing contraction with  $*h$ . It is important to bear in mind, however, that in Twareg  $ə < *ā$  is far less exposed to omission than  $ə < *i/ū$ , even if the syllable structure permits it.

(6) The question remains to be investigated whether the labialization found in certain consonants in Tashelhit and Kabyle might not be the result of contact with a proto-Berber short  $*ǃ$ .

(7) The vowel of the masculine plural ending must have been  $*ā$ :  $*-ān$ . Twareg has a variant  $*-īn$  in the imperfect.

(8) For the short  $ā$  of Twareg pl. *ālsān*, *əmdān* see section on the reduction of long vowels.

It is impossible to give here an account of the numerous particular problems met with in the reconstruction of the whole Berber vocabulary. This paper only serves to outline the method. An interesting example from the nominal system would be the instrument noun with prefix  $S$ :

	'underskirt'	'garment'	'sack'	'key'
Twareg	<i>āsəgbəs</i>	<i>āsəlsu</i>	<i>āsaməd</i>	<i>āsaru</i>
Kabyle	( <i>asərgəl</i> 'lid')	( <i>asəḍsu</i> 'front teeth')	( <i>asag<sup>w</sup>əm</i> 'jar')	<i>tasarut</i> (f.)



Tashelhit	( <i>tasrg<sup>w</sup>lt</i> [f.])			<i>tasarut</i> (f.)
*proto-Berber	* <i>āsagbus</i>	* <i>āsalsuh</i>	* <i>āsahmud</i>	* <i>āsahruh</i>

*ā* in modern Twareg indicates that the vowel may be long although in normal speech it is short, *ǎ*. The penultimate vowel is *ə* in some dialects ('Umlaut?'), but perhaps *ä* in others.

CONCLUSIONS: Kabyle shows that the state (initial) vowel was long, although in Twareg it is often short—a very interesting documentation of the fact that it can by no means be taken for granted that Twareg is archaic in all cases.

#### REDUCTION OF ORIGINALLY LONG VOWELS

In certain cases the modern Berber dialects do not agree in vocalization, some having a full/long vowel, others a central/short vowel in the same position. Only extensive comparison can help to decide whether in any particular case we have lengthening of a proto-Berber short vowel or reduction of a long one. We have already noted three cases which favour the latter possibility.

(1) In the perfect plural of the verb types *āls* and *əmdu* we have in Twareg a distinction between *əlsān* and *alsān* (simple and intensive perfects) as against the unique full vowel of all other dialects. As the distinction of a specific intensive perfect seems to be a Twareg invention, at least in its peculiarly Twareg form,<sup>6</sup> it is probable that the simple perfect form *əlsān* represents a reduction of *əlsan* by analogy with the strong verbs (simple *ikrās*, *ikrās* in all dialects as opposed to intensive *ikrās* in Twareg).

(2) In the imperfect the 1st and 2nd person sg. suffixes of verbs of type *āls* are, in all dialects, *-əγ*, *-äγ*; *-əd*, *-äd*. Rather than a reduction of *i* we have a simple case of analogy with those persons which have no suffix, which have also lost their final *-i*.

(3) The state vowel of the free state sg. may be short in Twareg nouns and certain facts make it appear probable that here we have a case of reduction. Not all Twareg nouns undergo this abridgment and some of those which do only do so facultatively. The state prefix seems to be simply the preposed demonstrative pronoun *a* (*u*, *o*), full/long in all dialects.

(4) The vowel of the annexed state prefix (*wə/yə*) is short/central in all cases, likewise abridged from *wa* (*wu*, *wo*). Similar reductions are also known in some

<sup>6</sup> For possible intensive perfect forms in Northern Berber cf. Picard (1957).

Southern Twareg composite demonstratives; e.g. *wadi* (< \**wādi* by 'Umlaut'), Tahaggart *wadi*.

(5) The conjugations with full penultimate vowel before consonant group (i.e. in closed syllable) are not universally attested, nor fully developed even in Twareg. This seems to be due to reduction of the vowel; e.g. Tahaggart *dukkal* 'to be gathered' forms the intensive imperfect *itidakkūl* (cf. *itidūbūn* from *dubən*), causative *səddakkal* perfect *isdakkūl* (cf. *səddubən/isdabān*).

#### LENGTHENING OF ORIGINALLY SHORT VOWELS

Some full vowels must, however, rather be secondary, that is lengthened forms of their central/short counterparts. All the cases that I have been able to establish are Twareg.

(1) The Twareg intensive perfect of the strong tri-literal verb must have a lengthened *ā* < \**ā*, the distinction between simple *ikrās* and intensive *ikrās* being a peculiarity of Twareg.

(2) For the same reason the first vowels of the intensive imperfect must be secondary in most conjugations. Thus Twareg *ikārrās* (neg. *ikərrəs*) corresponds to Ghadamsi *ikārrās* (*ikərrəs*), with short vowels only. Tashelhit/Kabyle *ikrrs*, *ikərrəs* must, however, be the result of the coalescence of the positive and negative forms. Twareg *itābārāg* (from *bārāg* 'to boast') corresponds to Kabyle *yəṭṭənyəl* (from *ənyəl* = *ənyəl* 'to pour'), Ghadamsi *ittāmāgār* 'to be thrown'. The first vowel is central except in Twareg where its length appears to be secondary. As for the second vowel, all dialects tend to show that it was originally short. The last is, however, full/long in most dialects, although not, for instance, in Ghadamsi. It seems necessary to assume that it was long in proto-Berber.

(3) Twareg seems also to have lengthened vowels in the nominal system, for example the penultimate of the verbal adjective of the type *āmākras* 'who binds'. The corresponding Northern Berber form regularly has *ə* in this position.

#### CONTRACTION OF ORIGINALLY LONG VOWELS WITH \**h*

This phenomenon can be observed particularly well in the final syllable of words, since Berber shares with Semitic the property of very frequently having a full/long vowel before the final radical.

Thus the Twareg verbal adjective *emalki* < \**ēmālkīh* 'who despises' (from *əlku*) must be of the same type as *eməṇir* < \**ēmānhīr* 'who guides' (from *əṇər*), *ānəbdid* 'who takes care' (from *əbdəd*). The negative perfect of the verbs of our table has likewise a full *i(e)* before the last radical in all Berber dialects: Twareg *wər-ikris*. *Wər-ilse* must then derive from \**wʔr-yulsīh*.

The material I have analysed above shows that in cases of contraction with *\*h* there is no observable difference in the result dependent upon the quantity of the vowel involved, *\*ih* and *\*ih* both becoming *i(e)*, etc.

TABLE

Dialect	Type Root	Imperfect/perfect: 3rd pers. masc. Singular	Plural	Meaning
Tw.	əkrəs	ikrəs/ikräs	<sup>i</sup> əkrəsän/əkräsän	'to tie'
Ghad.	*/krs	yäkrəs/ikräs	äkrəsän/əkräsän	„
Kab.		yəkrəs/yəkrəs	kərsən/kərsən	„
Tash.		ikrs/ik <sup>w</sup> rs	krsn/k <sup>w</sup> rsn	„
*proto-B.		yakris/yukras u	akrisan/ukrasan u	
Tw.	agəm	yağəm/yugām	ağmin/uğämän	'to draw water'
Ghad.	*/hgm	yağəm/yugām	ağmən/uğämän	„
Kab.		yag <sup>w</sup> əm/yugəm	ag <sup>w</sup> mən/ugmən	„
Tash.		yag <sup>w</sup> m/yug <sup>w</sup> m	ag <sup>w</sup> mn/ug <sup>w</sup> mn	„
*proto-B.		yahgim/yuhgam u	ahgiman/uhgaman u	
Tw.	äls	yäls/ilsa	<sup>ä</sup> älsin/əlsän	'to wear'
Ghad.	*/lsh	yäls/ilsō	älsin/əlsōn	„
Kab.		yəls/yəlsa	əlsən/əlsan	„
Tash.		ils/ilsa	lsn/lсан	„
*proto-B.		yalsih/yulsah	alsihan/ulsahan	
Tw.	əmdu	imdu/imda	<sup>ä</sup> əmdun/əmdän	'to complete'
Ghad.	*/mdh	imdu/imda	əmdun/əmdan	„
Kab.		yəbɖu/yəbɖa	əbuɖn/əbɖan	'to divide'
Tash.		ibɖu/ibɖa	bɖun/bɖan	„
*proto-B.		yamduh/yumdah	amduhan/umduhan	
Tw.	as	yas/yusa	<sup>ä</sup> asin/usän	'to arrive'
Ghad.	*/hsh	yaf/yufō	afin/ufōn	'to find'
Kab.		yas/yusa	asən/usan	'to arrive'
Tash.		yaf/yufa	afn/ufan	'to find'
*proto-B.		yahsih/yuhsah	yahsihan/yuhsahan	

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## DISCUSSION

PRASSE: I have had a small exchange of correspondence about this paper with my friend in Paris, Lionel Galand, and I should like to read a passage from his letter because he has a somewhat different position from mine as regards the role of the central vowel, the *schwa*, in Kabyle. He says: "I believe that [ə] as a phonetic element is much commoner in Kabyle than in Morocco, but I do not think that it possesses

the status of a phoneme. The conditions of its appearance and the place it occupies are in my opinion always predictable. The opposition between [əls] and [gər] is, if one limits oneself to the imperative, purely phonetic. If I had to write a lot of Kabyle I think therefore that I would adopt a phonemic transcription without the sign ə. I have not yet done so because I am a little less sure than in the case of Morocco and because I wish to avoid a premature decision". That is to say, he apparently does not share my view as to the possibility of distinguishing between words like əzɛd and zəɛd which I have quoted in my paper.

JEANETTE HARRIES: I find Mr. Prasse's paper very interesting. I have done no historical work myself and would only like to make two comments based on descriptive work. Firstly, I find that the notion of short vowels in a proto-language not being subject to any change in quality by adjoining laryngeals is not terribly realistic phonetically, since all of the short or long vowels I have observed occurring in present day Berber languages seem to be strongly influenced by at least one of the laryngeals, the 'ayn, and one would wonder if this could really be the case. The other comment is that I find that many of the descriptions of Berber languages as having short vowels, or vowels of a great many degrees of length, seem to reflect a great deal of the linguistic structure of the analyst. I have myself questioned this and experimented a little with sound spectrograms and the like to see if what we perceived as short vowels were really there, and found that in some cases we were perceiving vowels where there was absolutely no evidence on sound spectrograms of anything but a transition from one consonant to another. It was apparently the structure of our English which led us to expect that syllables had vocalic peaks, and I think that this perhaps accounts for a great many of the vowels that are transcribed in much of the Berber work. It would make one hesitate about reconstructing forms on the basis of the transcriptions.

VYČICHL Mr. Prasse's welcome study may, I think, be completed by taking into consideration the historical elements at our disposal for the study of the history of Berber forms. We have about thirty words of Punic origin, we have many Latin and Greek loan-words, we have hundreds of place-names of Graeco-Roman times the forms of which can be followed up to today. This enables us, if we start with Herodotus, to retrace the history of Berber vowels over a period of two and a half millennia, which is a considerable time. It is nearly half that which separates us from Old Egyptian (with five thousand years). There are some sound changes which we can observe. We know that practically from Punic times no considerable vowel changes have operated in Berber. We have words like *Agadir*, from *gādēr*, in which we can say that the vowels have remained constant. On the other hand in some cases, what was originally long *ū* has become long *i*. We have the town in Tunisia which is called in Latin *Clupea* and which is today in Arabic *Qlibya* (written *Kalibia* in French); here the long *ū* has become *i*. It is not an isolated case, I have about twelve such

examples. I shall quote you another: Latin *cepula* 'onion' has given *uqfil*. We must also take into consideration when studying the vowels the phenomenon of 'Umlaut', namely the influence of an *i* or a *u* on a preceding *a*. Thus from Latin *cicer* 'chick pea' modern Berber does not have \**akikər* but *ikikər*, because the *i* of the stem has influenced the initial former article. We also have a region where the *u* umlaut existed, that of Ghadames and the Djebel Nefusa today. There the Latin word *furnus* (or accusative *furnu*, as it was pronounced in later times) has given in that region not *afurnu* as in Morocco, but *ufərnu*, where the *u* has influenced the initial vowel. There is quite a long list of such words beginning with *u-* in the masculine form and *tu-* in the feminine form. A last point: one finds in Schilha, which is an umlaut language, forms without umlaut, as for instance *alim* 'straw', because here the *i* of the stem is an old *u* (cf. Mzab *lum*) and *u* in Schilha does not produce umlaut as it does in the Djebel Nefusa. We have other words like *asif* 'river', pl. *isaffən*, which is *suf* in the Mzab and *usəf* in the Djebel Nefusa.

PRASSE: I should like to give a very brief answer to Dr. Vycichl. We have already had a long correspondence about all these examples which he has produced in favour of different changes of the full vowels in Berber and I have examined them very carefully. I feel that there is no simple explanation to these changes. I admit that they constitute problems which must of course be solved, but I do not think that there is a unique solution. May I state it like this: no spontaneous change of the Berber long vowels can be proved to have taken place, for instance of *ū* to *ī* or of *ā* to *ū*. I think that these changes only occurred in certain phonetic circumstances. They were produced by dissimilation, for instance. Thus Twareg *əggəz* 'to go into' has an infinitive in Tahaggart *ūgūh*, but in Southern Twareg dialects you find a dissimilated form *īgūz*. In the same way you often find that *ū* has been dissimilated into *ī* because of the presence of a radical *wāw*.

BYNON: On this question of the long vowels, if you take a word like *ifilu*, which obviously comes from Latin *filum* 'thread', we find this in the variant forms *ifilu*, *afilu*, *ifili*, *ifulu*, *afulu*, and even *fuli*. In any case you have most, although not all, of the theoretically possible combinations. This demonstrates at least that these vowels are highly interchangeable. I think one of the reasons for this is perhaps that the vowels in Berber play a largely morphological role, and in a loan-word like this, if it did not fit an existing pattern—in other words if these vowels had no morphological 'meaning'—then they were much more liable to change in this way and 'UMLAUT', as you say, could play a role. Another thing is that you quoted the words *asif* 'river' and *alim* 'straw'. In the construct forms of both of these the *a* is 'constant', that is to say it does not fall (*wasif*, *walim*), and I think that this fact might have a lot to do with its unaltered quality.

VYCICHL: Initial *a* is constant in Chleuh in short words. If you have a word like

*ass* 'day' the *status constructus* is *wass* and not *\*uss*; because otherwise the word would be too much disfigured.

PETRÁČEK: A few weeks ago I sent to the printers an article entitled "Die Grenzen des Semito-Hamitischen : zentralsaharanische und semito-hamitische Sprachen in phonologischer Hinsicht" in which I found myself obliged to re-examine the theories of Basset and of Rössler who tacitly assume the proto-Semitic system for, among others, proto-Berber. I wondered whether we were not dealing here with a *petitio principii* since they did not give any explanation. But I am very happy to see here the explicit arguments put forward by our colleague Prasse, which will be very useful for us comparativists.





## HAMITO-SEMITIC AND THE PRE-CELTIC SUBSTRATUM IN IRELAND AND BRITAIN

G. B. ADAMS

Lest it seem presumptuous to offer to a Colloquium on Hamito-Semitic Comparative Linguistics a paper on the possibility of a language of this family being substratic to the Celtic languages of Ireland and Britain, let me begin by recalling that down to the middle of last century the Celtic languages themselves were seriously regarded in some quarters as being derived from Semitic. The Indo-European (IE) origin of Celtic was only tardily recognised and though it was already proposed by Pictet (1837) and in a vaguer way even earlier by Edward Lhuyd (1707), it required the penetrating analysis of Zeuss (1853) to clinch the matter. His demonstration that Celtic was undoubtedly Indo-European killed the theory of derivation from Semitic or Hamitic sources stone dead for almost half a century. When the theory of a connection with Hamito-Semitic (HS) was revived in a new form by John Morris Jones (1900) the connection proposed was that of the influence of a Hamitic substratum on the development of insular Celtic syntax. This idea belongs to the theory of areal linguistics developed later by Ernst Lewy (1942), rather than to that which, working from the genealogical relationships between languages, has established the existence of the Indo-European and Hamito-Semitic and various other language families.

Morris Jones incurred the derision of many Indo-Europeanists and his ideas found scant support in Ireland where they seemed to hark back to the fantastic ideas on language derivation prevalent in the 18th century and to be an affront to the patriotic view of Irish as Ireland's national language from the time of the earliest inhabitants. Even yet some Irish Celticists who are moved by purely academic considerations look with the greatest reserve on any attempts to probe Ireland's pre-Celtic linguistic past and are loth to attribute anything to substratic influence. Yet such an influence there must have been—have we not the evidence of Gaelic influence on the development of the English now spoken in Ireland to support this belief?—unless we are to believe that the ancient pre-Celtic population consisted entirely of deaf-mutes or was struck dumb in amazement for a few generations by the arrival of pure Celtic paragons of loquacity. Continental scholars have been less inhibited and Pokorný (1927-1930) lent his support in a long series of articles. The weight of evidence was on the syntactical side and there the matter rested for a

generation. Celticists had too much else in hand for periods that are well documented and in most cases they had not the necessary knowledge of Hamito-Semitic languages to pursue the matter further. Hamito-Semiticists for their part were even less likely to be acquainted with Celtic and were in any case too much involved in establishing interrelationships among surviving and early recorded languages of North Africa and south-west Asia to consider the possible influence of extinct members of their family on the present languages of north-west Europe.

A fresh start was made with the publication of *Das Verbum in den Sprachen der Britischen Inseln* by Prof. Heinrich Wagner (1959), now professor of Celtic and General Linguistics in the Queen's University of Belfast, whose *Linguistic Atlas and Survey of Irish Dialects* (1958-1969) is a monumental contribution to the study of Gaelic dialectology in present-day Ireland. In the former work he discusses in detail parallels in the structure and use of the verb in Celtic, Berber, Basque, English and French, and restates the case for a common substratic link between these modern Atlantic languages.

Progress on the lexical side towards establishing links between Celtic and Hamito-Semitic has been less marked, and even some Basque etymologies for Celtic words that were proposed some years ago have turned out on closer examination to be borrowings in the opposite direction. The untimely death of Vendryes, when his etymological dictionary of Old Irish (Vendryes 1959-1960) had reached only the letters A, M, N, O, P, has deprived us of a full published list of those Irish words for which no sure IE etymology has yet been established. Even for the letters he published, he often makes no attempt to seek other sources which might in some cases be HS, but at least his dictionary provides a limited corpus of Irish material to which Hamito-Semiticists can turn if they seek from their side for lexical links in Atlantic Europe.

The whole question of pre-IE substrata in insular Celtic is now seen to be more complex than was envisaged by Morris Jones. For Hamito-Semitic it raises the question of pre-HS substratic relics in Libyco-Berber. Scholars who have been working on the question of pre-Romance elements in the languages of western Romania have come up with the idea of certain prehistoric language areas in Europe, of which the Alpine (or Rhaeto-Tyrrhenian) and the Eurafrican are perhaps the best authenticated. The existence of a very early Eurafrican language area is assumed from the occurrence in Berber, Hispano-Romance, Basque and sometimes Gallo-Romance, of certain words for which no satisfactory etymology can be provided from either Hamito-Semitic, Indo-European or the Caucaso-Pyrenean stock which now seems to be emerging as the basic component of Basque.

It is unnecessary to reiterate here in detail the kind of links, mainly syntactical and so far as research has gone only rarely lexical, that have been tentatively established between insular Celtic and its supposed HS substratum. In a paper entitled "Language and Man in Ireland", (Adams 1969-1970), I have listed in the bibliography those authorities on prehistoric western Mediterranean languages not

mentioned here. What I wish now to do is to consider first some chronological aspects of possible contact between insular Celtic and Hamito-Semitic and then some phonological aspects.

The earliest written evidence for IE is that provided by Hittite records of c. 1900 B.C. and Mycenaean Greek and Mitannic Indo-Aryan of c. 1400 B.C. On the basis of West Finnic loanwords in Hittite, Ronimois (1957) places contact between the western Finns and the Hittites shortly before 2000 B.C., before the latter had migrated round the western side of the Black Sea to settle in Anatolia but after they had separated from the main IE stock, which must have happened not later than about the middle of the third millennium B.C. At that time Egyptian had been a written language for some 500 years and Akkadian was just coming into use in Mesopotamian records. Though western Semitic records did not begin till the middle of the second millennium B.C. the existence already of the separate Semitic languages may be inferred, so that the break-up of Common Semitic can hardly be put later than 3000 B.C. (and may in fact be much earlier), while the separation of Semitic and Egyptian must lie even further back, probably in the 5th millennium B.C. Though the other branches of HS are not attested from comparable dates, it seems reasonable to suppose that they too separated from one another about the same time as Egyptian and Semitic. This means that their separation predates the earliest Neolithic settlements in Ireland. Until recently these were placed at c. 2300 B.C. by archaeologists but their researches during the last dozen years have enabled them to push this date back considerably. The most recent evidence from C 14 tests on charcoal associated with Neolithic pottery finds at Ballynagilly, Co. Tyrone, point to a date<sup>1</sup> c. 3700 B.C. and as this is an inland site we may assume that western Neolithic farmers had reached coastal areas of Ireland not long after c. 4000 B.C. It follows then that even if their language was HS, which is very far from being certain, we would not be concerned with some original Common Hamito-Semitic but with one or other of its known branches or possibly with some extinct branch whose speakers penetrated at a very early date to Atlantic Europe where their language flourished till Iron Age times and then became extinct. Leaving this possibility aside for the moment, the three branches of HS with which we may be concerned, whether they came in early Neolithic times or later, are Libyco-Berber, Egyptian and North-West Semitic. Cushitic and Chadic are not directly involved, but they could be indirectly involved in the sense that as language groups on one periphery of the ancient area of HS expansion they may preserve archaic features that occurred in whatever HS language reached Atlantic Europe, and such features might or might not have survived by carry-over into the insular Celtic languages. In the same manner, in respect of their basic IE component, the Celtic languages on the western periphery of IE expansion quite often share with Indo-Aryan on its eastern periphery features that have been lost in the intervening areas.

<sup>1</sup> I wish to thank Mr. L. N. W. Flanagan, Keeper of Antiquities, Ulster Museum, Belfast, for this information.

The lower dates for the possible settlement of HS-speakers in Ireland and western Britain, if we exclude Phoenician traders from c. 1000 B.C. onwards, who may not have been numerous enough to establish their language, is c. 1500 B.C., i.e. about the beginning of the Irish Middle Bronze Age, which seems to have been an era of trading expansion and possibly population expansion outwards from Ireland rather than of movement inwards. This brings us within the time range of the western megalithic cultures whose ultimate sources seem to lie in the Aegean and especially Crete, where some scholars have sought to explain the Linear A inscriptions as Semitic, though others link them with Hittite and other Anatolian languages. If populations of eastern Mediterranean origin reached Ireland and western Britain, say between 2500 and 1500 B.C., in sufficient numbers to establish their language in the megalithic age, it was perhaps more likely to have been some early form of North-West Semitic rather than Egyptian, given what we know of the seafaring activities of these two peoples. Faience beads of 14th century B.C. Egyptian origin have indeed been found in Irish excavations but they could have come in by trade and do not necessarily imply any settlement of Egyptians so far west.

Archaeological indications, however, point to the western Mediterranean as the main source of Neolithic and Early Bronze Age immigration during the fourth and third millennia B.C. In the early phases of this movement the languages involved were almost certainly not HS, unless some unknown and now extinct branch of this family reached the western Mediterranean lands ahead of Libyco-Berber. To some extent, therefore, Celticists and Berberists are concerned not with direct contact between their languages in prehistoric times but with parallel contact of their respective languages with some—presumably Eurafican—language or language group which was the common substrat to both. This being the case, lexical links would belong to whatever pre-HS component can be identified in Berber and not to its main HS component. This reduces the general HS interest and the chances of identification being substantiated from other branches of HS, but it means that the insular Celtic languages may provide some help to Berberists in solving some lexical problems for which HS comparisons have not provided an answer. There remains of course the question of whether Libyco-Berber was spoken early enough in any of the western Mediterranean lands for its speakers to have taken part on a large scale in the later stages of northward migration in Atlantic Europe during the third or early second millennium B.C. On this question Celticists would welcome a firm statement of the latest views of Berberists as to when Berber took shape in the Maghreb and as to whether—and, if so, when—it was ever spoken in the western Mediterranean islands and the south-western mainland of Europe.

The next stage in our chronological investigation concerns the dating of Celtic arrival in Britain and Ireland, and the lower limits of any possible survival of pre-Celtic languages there. We may reject out of hand the back-dating of the Celtic invasions to the first half of the second millennium B.C., which derives from the efforts of the 10th and 11th century A.D. Irish synthetic historians to link Irish

history to the Old Testament and classical sources, supplemented by the willingness of 19th and some 20th century archaeologists to clothe their speculations in the garb of history. Such dating is at variance with what is now known of Celtic and general IE history, and the Celtic settlements in Britain and Ireland may safely be attributed to the last half of the first millennium B.C. The possibility of some form of proto-Celtic or non-Celtic IE, like the very early Hispanic IE traced in north-west Spain and Portugal (Tovar 1949; Schmoll 1959, *passim*) tentatively attested by some river names in Britain (Reader's Digest Atlas 1965, 124), but not as yet in Ireland, does not affect the issue, since such IE forerunners of the Iron Age Celts were doubtless a minority absorbed either by their predecessors or their successors. We must therefore reckon with the possible survival of non-Celtic languages deriving from Neolithic and Bronze Age times until as late in some areas, particularly in Ireland, as the beginning of the Christian era. The possible survival of a non-Celtic and non-IE language in parts of north-east Scotland as late as the period of the Pictish ogham inscriptions (Macalister 1940; Jackson 1955) is, however, a separate matter, since the source in this case is more likely to lie in the survival of north European Mesolithic language, as Wagner (1964) has recently shown, than in Neolithic or Bronze Age languages from the Mediterranean area.

Within the more or less certain chronology of Celtic arrival we are still faced with disagreement as to whether Q-Celts (speaking a language from which, in the main, Irish is descended) or P-Celts (speaking a language from which Welsh is descended) arrived first. According to the older view the former preceded the latter by several centuries in a general movement across Gaul to Britain and then onwards to Ireland, or in some cases direct from Armorica to Ireland. O'Rahilly (1946, 15-17 *et passim*), however, produced a scheme of Celtic invasions of Ireland which brought the P-Celts in first, partly via Britain and partly direct from north-west Gaul, to be followed c. 150-50 B.C. by the Q-Celts whose dialect forms the main component of the Irish language of historical times, though O'Rahilly held that it also contained P-Celtic elements taken over from the dialects of other Celts already established in Ireland (O'Rahilly 1946, 205-207; 1935). His attempts to trace these Q-Celts to south-eastern Gaul were widely rejected and he himself rejected the traditional Irish view that the presumably Q-Celtic Goidels (Gaels) came from Spain. Since then, however, it has been shown that the Celtiberi of north-central Spain spoke a Q-Celtic language, though the Celtici of Portugal and western Spain were P-Celts (Tovar 1949, 21-60, 96-126). The former appear to derive from a Celtic invasion of Spain in the Hallstatt Iron Age, the latter from migrations of the later La Tène Iron Age. In this way we can see how Q-Celtic, reaching Spain before P-Celtic, could nevertheless have reached Ireland from that quarter much later, when P-Celts were already in partial occupation of Ireland. The matter is still *sub judice*, but if we admit the possibility of Q-Celts passing through and sojourning in Spain for some centuries before reaching Ireland, then the whole question of possible Eurafrican and Libyco-Berber loan-words in Irish is seen in a fresh light. They could have been mid-1st millennium B.C.

loans acquired on the Continent and brought to Ireland, rather than loans acquired from a pre-Celtic language spoken in Ireland. In fact, both kinds of borrowing could have occurred. The Roman conquest of northern Spain, following their annexation of the Carthaginian dominions in southern Spain c. 200 B.C., could have provided the occasion for a migration of Q-Celtic refugees to Ireland. In some parts of Ireland P-Celts seem to have rapidly adopted Q-Celtic; in others a P-Celtic language known in Irish tradition as *Iairnbhéarla* 'language of the Érainn' (a people in Munster) appears to have survived till the dawn of historical times. For further discussion of this see O'Rahilly (1935; 1946) and Adams (1969-1970), in which I suggest that preferences among the surviving pre-Celtic population as between rival Celtic dialects may have helped to swing the balance in favor of Goidelic.

To summarize, within the span of the last four millennia B.C. we must reckon with three possible sets of circumstances that could result in Celtic and Berber sharing certain common elements of vocabulary, each of these having several possible subdivisions, depending on various geographical and chronological factors. These are as follows:

- (1) Eurafrian words, whose diffusion in the Atlantic countries was due to Western Neolithic expansion in the fifth and fourth millennia B.C., could have been borrowed independently by Libyco-Berber at such time as Berber took shape in the Maghreb (on this point of chronology I seek the help of other members of the Colloquium) and by insular Celtic at some time from the middle of the first millennium B.C. down to about the beginning of the Christian era, if not later. In this group we must reckon with divergent phonological and semantic developments in the source language, or related languages if there were more than one, before contact was made with Libyco-Berber immigrants in one case and Celtic immigrants in the other, assuming in the latter case that some form of Eurafrian survived later prehistoric immigrations in some parts of either Ireland or Britain until the Celts arrived.
- (2) Words of Libyco-Berber or other Hamito-Semitic origin belonging to languages of this family established in Ireland and/or Britain in late Neolithic and Early Bronze Age times could have been borrowed into Irish and/or Brythonic as the case may be at the same period as above, assuming that such languages were established in some areas and survived till Celtic times.
- (3) Words of Berber origin, whether ultimately of Hamito-Semitic or of Eurafrian or other non-HS etymology, could have been borrowed into Spanish Q-Celtic, either direct or through the medium of Basque, during the middle centuries of the first millennium B.C. and brought to Ireland as part of the basic Q-Celtic component of the later Irish vocabulary, probably during the last couple of centuries B.C., assuming that some of the Q-Celtic settlers in Ireland came from Spain as Irish tradition averred.



Each of these word-groups is hedged about with assumptions and qualifications which we are not yet in a position to resolve, but I believe that they do represent a fair statement of the possibilities that must underlie and divide any corpus of common lexical items that may exist, whether it prove to be large or small, into three basically different categories. Each of these may have quite separate laws of phonological correspondence, because of different linguistic and chronological starting-points and different intermediate contacts. From the Celtic side we can trace the separate Celtic phonological systems, by the accepted methods of IE comparative linguistics, backwards to converge in Common Celtic as it must have existed in central Europe sometime c. 1000-700 B.C., if not earlier, i.e. before the Celts, either on the Continent or in the British Isles, had made contact with any HS or other pre-IE language in western Europe. Tovar (1962) has attempted an assessment of the Iberian sound system, which was probably of Eurafrian origin, in eastern Spain about the middle of the first millennium B.C., and André Basset (1952, 5-8) has given a *système fondamental* for Berber based on the phonologies of the present Berber dialects, though without relating it, so far as I am aware, to any form of ancient Libyco-Berber or to other HS languages. This has been done in general terms but without chronological particularization by Marcel Cohen (1947). Here again Celticists need the assistance of Hamito-Semitists to establish some kind of phonological chronology for the whole HS family in general and for the Libyco-Berber branch in particular which can be set against the gradually changing state of early Celtic phonology during the first millennium B.C.

Having summarized the theoretical considerations that govern possible lexical links between Celtic and Hamito-Semitic, it remains now to illustrate them by a few concrete, though still in part tentative, examples. To keep the length of this paper within reasonable bounds I will discuss only one word in some detail and mention some other Irish words of pre-Celtic origin in passing with references to where discussions about them may be found. The particular example in question belongs to the third category of words listed above and has the special interest of being a HS word which, having gone the rounds of Berber, Basque and Irish, has produced derivatives that are still current in the English dialects now spoken in Ulster. *Buz-zacker* 'heavy blow' is its most fully anglicized derivative, with the English agential suffix *-er*, used here almost in a personificatory sense as in words like *corker*, *wheeker*, and with voicing of the medial sibilant that suggests a fairly long incorporation into the English vocabulary. Closely connected and more recently borrowed from Irish are the forms *bossock* and *buthog* 'blow on the ear', recorded from Co. Tyrone, the voiceless *th* in the latter form being an interdental substitute for the broad ambidental *s* of Ulster Irish, a phonetic shift that occurs sporadically in present-day Donegal Irish itself. The suffix *-óg*, anglicized as *-ock*, *-ack* or *-ug*, which appears in the Irish forms *bosóg*, *basóg* 'slight blow with the hand', from which these forms are derived, comes from Old and Middle Irish *-óc* which is a take-over from Old and Middle Welsh *-oc*, earlier *-awc*, from Ancient Celtic *-acos* (with long *a*). The

Gaelic form of this suffix, *-ach*, is used in Irish to form adjectives and nouns from other nouns, while its doublet *-óg* usually forms diminutives, though the latter sense is now sometimes lost. The word *bos*, *bas*, 'palm of the hand', to which it has been added, has no known IE cognates. Its particular sense is opposed to the native word *lámh* 'hand, including the forearm', which with Celtic loss of IE *p* is cognate with Latin *palma*, Greek *palámè* 'palm of the hand' and whose extension of meaning has necessitated a new term for the palm. It is suggested that Irish *bos* is cognate with Basque *bost* 'five' and that both words are loans, with divergent developments of meaning, from an older form of the HS word seen in Berber *afus* 'hand' (Pokorny 1930, 111; Hubschmid 1965, 147). In the latter form Berber *f* corresponds to Egyptian and possibly common HS *p* (Cohen 1947, 107, No. 148). The change of initial *p* to *b* is Basque, cf. Basque *bake* 'peace' from Latin *pace(m)*, and possibly also Iberian (Larrasquet 1939, 72), so that Irish *bos*, if it is connected with Berber *afus*, must have passed into Celtiberian or some other variety of Q-Celtic through the medium of Basque or Iberian which in turn must have acquired the word, presumably with some special semantic connotation—perhaps as a counting term—from an archaic stage of Libyco-Berber in which HS *p* had not yet become *f*. If members of the Colloquium can give their assent and support to this possibility, then my tenuous contribution to its deliberations will not be entirely unjustified.

A few other Irish words of non-IE origin may be mentioned with reference to discussions on them, viz. *carn* 'heap of stones' and *carraig* 'rock' (Hubschmid 1953, 84; 1965, 41, 98) and *móthar* 'thicket, place overgrown with brushwood' (Hubschmid 1953, 36), both possibly Eurafrican rather than HS; *móin* 'turf' (Vendryes 1959-1960); *iarann* 'iron' (Benevise 1956, 279), which is possibly of Alpine or Caucasian origin; *tulach* 'small hill' (Hubschmid 1953; Adams 1956, 15-16, 23), which alone among these few words may be HS, cognate with Arabic *tel*.

There is a special category of terms in Irish that are untypical of IE languages but find their parallel in HS, and they exist in Irish on a scale and with such individual characteristics as to suggest that they are not literary borrowings from biblical Hebrew but popular formations arising by colloquial translation and adoption during a period of bilingualism when Celtic was replacing some language of HS type. These are the terms in which something is described as the *mac*, 'son', of something else. Below is a short list giving the Irish term followed by its English translation, its literal word-for-word meaning (in brackets), and in some cases where known to me an exact or near equivalent in one or other HS language (in Roman transcription).

*mac báis* 'criminal' (lit. 'son of death'), Hebrew *ben maweth*. This particular example could be a literary borrowing.

*mac alla* 'echo' (lit. 'son of a cliff'), cf. Arabic *ibn jebel* 'son of a mountain'.<sup>2</sup>

<sup>2</sup> I have seen this Arabic term but cannot now trace the source, so the form given here is subject to verification.



*mac léighinn* 'student' (lit. 'son of learning'). This particular example dates to c. 6th or 7th century, since *léigheann* (from *legenda*) referred originally to the written Latin learning of the Church as opposed to the traditional oral learning of the *fili*.

*mac seó* 'show-boy, trickster' (lit. 'son of show, show-off'). This particular example is of fairly recent origin since the second word is from English *show*.

*mac soipín* 'upstart' (lit. 'son of a wisp of straw').

*mac eaglaise* 'cleric' (lit. 'son of a church').

*mac tíre* 'wolf' (lit. 'son of land').

*mac an droma* 'the fourth finger' (lit. 'son of the back, ridge').

*mac raide* 'gun, musket' (lit. 'son of a kick' or 'son of thrusting').

*mac samhla* 'copy' (lit. 'son of resemblance').

*mac an abair* 'the fourth finger' (lit. 'son of the marsh'; cf. *mac an droma*).

*mac foghlama* 'student' (lit. 'son of learning', cf. *mac léighinn*).

*mac mallachtan* 'scapegrace' (lit. 'son of cursing').

*mac mádha* 'king of a suit other than trumps, at cards' (lit. 'son of fortune' or 'son of trumps').

*mac an óil* 'innkeeper' (lit. 'son of the drink').

*mac ealaíon* 'artist' (lit. 'son of skill').

*mac fuirmhidh* 'poet of the sixth degree, i.e. in early Irish society' (lit. 'son of propounding' or 'son of arrangement').

*mac doirche* 'bastard' (lit. 'son of darkness').

*mac soirche* 'legitimate son' (lit. 'son of brightness').

*mac scríne* 'incestuous son', or 'son of a cleric' (lit. 'son of a shrine').

*mac faosamh* 'protégé, adopted son' (lit. 'son of protection').

*mac raithnighe* 'brat' (lit. 'son of bracken').

*mac imreasain* 'pupil of the eye' (lit. 'son of contention'); cf. Hebrew *bath ayin* 'pupil of the eye' (lit. 'daughter of an eye'). The Hebrew use of *bath* rather than *ben* in this case may be connected with the fact that names of parts of the body that go in pairs are feminine in Hebrew. The same is generally true in Irish, e.g. *lámh* 'hand', *cos* 'foot', *cíoch* 'breast', *cluas* 'ear', *súil* 'eye', *uille* 'elbow', *guala* 'shoulder', all feminine and of IE derivation but despite this support the gender correspondence has not been carried over in the formation of *mac imreasain*, though in fact *inghean* (now *iníon*) 'daughter' is used in a few terms of this type, e.g.

*inghean gaoithe* 'breath' (lit. 'daughter of wind').

*inghean na hoíche* 'a kind of boil' (lit. 'daughter of the night').

*inghean carbaid* 'illegitimate daughter' (lit. 'daughter of a litter or coach').

To the above list may be added a few more examples from Old Irish:

*macc meda* 'drunkard' (lit. 'son of mead').

*macc bronn* 'species of metrical composition' (lit. 'son of a bosom').

*macc greche* 'kernel' (lit. 'son of a nutshell').

*macc saele* 'salve' (lit. 'son of spittle').

I have given a fairly long list of these in the hope that it may be possible to find parallels in one or other of the HS languages. The same type of formation evidently occurs in Berber in terms such as *ou-dari* 'mon compatriote' with plural *ait-dari* (Laoust 1936, 302); *ou-regg* 'habitant de la plaine', in which *ou*, *ait* are the construct forms of *aouwi*, *aouwit* 'son, sons'. The use of the plural form *Ait* in Berber tribal names, e.g. *Ait Frah*, *Ait Ouriaghel*, has an interesting parallel in the use of the form *Uí* in many early Irish tribal names, e.g. *Uí Máine*, *Uí Faoláin*, *Uí Néill*, *Uí Briúin*, *Uí Dróna*, some of which have become territorial names and which are not to be confused with later family names such as *Ó Faoláin*, *Ó Néill*, in which *Ó* 'grandson, descendant' is the singular of *Uí*. Earlier forms of these words were *Ua*, still sometimes used, plural *uai*, from Archaic Irish *aue*, plural *ai*. This word has a perfectly good IE etymology and appears in other IE languages as Latin *avus* 'grandfather', Gothic *awo* 'grandmother' and the first syllable of Welsh *ewythr* 'uncle'. The common thread of meaning would appear to be 'a relative two steps away from oneself', but it may be queried whether contact between some early group of Q-Celtic-speakers and speakers of some offshoot of Berber may not have been the factor that caused the Irish *aue* 'grandson' to connote a relationship of descent, like Berber *aouwi* 'son', to which it stands close in pronunciation (though here chronological factors would have to be considered), rather than the relationships of ascent that are found in Latin and Gothic. The parallel in the use of the plural forms, Berber *Ait* and Irish *Uí*, in the formation of tribal names is also suggestive of cultural and linguistic contact at some time in the mid-first millennium B.C. when Celtic-speakers and Berber-speakers might have made contact somewhere in Spain. Is there any evidence for the presence in Spain of Berber-speakers in Carthaginian or pre-Carthaginian times? If so, this example falls within the third category of lexical links discussed above.

Let us turn now to a curious parallel or semi-parallel between Irish and Hebrew in which both syntactical and phonological factors are involved and which extends, at least in some of its particulars, to other HS languages. It is one that illustrates the limitations no less than the wide ramifications of Celtic/Hamito-Semitic comparisons and shows how necessary it is to consider each facet of a complicated parallel on its own merits and its own structural and chronological level so as to disentangle what is purely fortuitous from what may be evidence of some degree of language contact.

We are concerned in this instance with the situation that arises when, according to IE grammar a noun in the genitive case is qualified by yet another genitive, and this in turn by perhaps another. In English we can speak of one's *wife's mother's cat's kittens* or of the *key of the drawer of the desk of the secretary of the Board of Governors*, and the same is true of most other European languages, whether they retain the inflected genitive as in the first example or have replaced it by a prepositional construction as in the second example, e.g. in the Romance languages. The same type of construction existed also in older Irish but it has increasingly been replaced by one in which only the last noun in the series retains the marks of the

genitive case, viz. the genitive inflection, the genitive form of the definite article (if it is so defined), and whatever initial mutation of the noun is caused by this article according to the gender and number of the noun. Intervening genitives, including adjectives qualifying them, lose their genitive case form and revert to an uninflected common case form, singular or plural as the case may be; lose their definite article, if any, along with the specific form of mutation caused by it; and suffer lenition of their initial consonant in so far as this is not inhibited by a homorganic final consonant in the preceding word, e.g.

*eochair an dorais* 'the key of the door',  
*eochair dhoras an toighe* 'the key of the door of the house',  
*eochair dhoras theach an mhúinteóra* 'the key of the door of the house of the teacher',  
*eochair dhoras theach mhúinteóir na scoile* 'the key of the door of the house of the teacher of the school'.

This is very close to what happens with the Hebrew *smichut* or construct form in a similar situation. Beginning with an actual example of a series of genitives from Isaiah XXI:17, let us strip it down stage by stage in reverse order to the above example:

*šə'ar mispar-qešeth gibbôrey bhəney-qedhar*,  
*šə'ar mispar-qešeth gibbôrey ha-banîm*,  
*šə'ar mispar ha-qešeth ha-gibbôrîm* (last word taken as adjectival),  
*šə'ar ha-mispar*.

If these steps are followed from the simple phrase of two nouns to the more complex it will be seen that the addition of each extra noun in what would originally have been the Semitic genitive form causes the preceding one to lose its definite article, if any; to assume the construct form, if different from the common form; and to suffer lenition of such consonants as are affected by a preceding final vowel. How does it come about that Irish, an Indo-European language, bears such a striking resemblance to Hebrew, a Hamito-Semitic language, in this kind of grammatical situation? And how much of this resemblance is purely fortuitous or attributable to substratic influence?

The definite article was not an original part of IE grammar but evolved at different stages in the development of the separate IE language groups and some IE languages have not acquired it yet. Irish and Welsh agree with Hebrew in having only a definite article but no indefinite article. They also agree in placing the *nomen rectum* (the genitive) after the *nomen regens*, and in placing the definite article only before the latter. This much extends to other HS languages and in so far as there may be any question of external influence at all in the development of this part of insular Celtic grammar the source would seem to lie in some form of HS. Of course, the similarity

might be due to pure chance. The omission of the definite article in each penultimate genitive when another is added is more distinctive and extends to Arabic and Welsh as well as to Hebrew and Irish, though in the former pair the mutational and inflexional changes that occur in the latter pair are lacking, e.g.

Egyptian Arabic:	<i>baab ilbeet</i>	<i>baab beet ilmudiir</i> (Mitchell 1956, 16)
Welsh:	<i>drws y tŷ</i>	<i>drws tŷ y rheolwr</i>
	'the door of the house' 'the door of the house of the manager'.	

In those cases where lenition of the initial consonant of an intermediate construct noun occurs it has been induced both in Hebrew and in Irish by a final vowel in the preceding word, but whereas in Hebrew this vowel still stands, in Irish it was lost in the transition from Ancient Celtic to Archaic Irish, i.e. c. 5th century A.D., unless of course we assume that the present incidence of lenition in such cases is not historically conditioned but has come about by some form of analogical distribution. The rule given above is that of present-day Ulster Irish (Ó Searcaigh 1939, 31) but a slightly different rule was followed in early modern Irish and may be found in the grammatical tracts used in the bardic schools where it was known as *sléagar*, a technical term whose derivation has not yet been explained (McKenna 1944, 262-270). According to this rule a genitive that would ordinarily be lenited loses its lenition, while one that would ordinarily be unaffected or eclipsed (i.e. have nasal mutation) has lenition instead. The Greek Septuagint version of Isaiah XXI:17, is *tò katáloipon tòn toxēumáton tòn ischyron huiōn Kedár*. If we can imagine Greek being influenced by Semitic idiom to the extent of expressing this as *katáloipo-toxēumato-ischyro-huiēis (toū) Kedár* (either with or without the definite article that is used in modern Greek before personal names), i.e. making all the words except the last genitive into a long compound whose elements are arranged in the Semitic word-order, we would have an approximation in IE word-forms to the Semitic idiom. If, further, we imagine that the pre-Celtic peoples of Ireland might have made some similar attempt to render their own phraseology into Celtic words, we can see that the final stem-vowel in the IE elements of such a compound would be the factor that would eventually induce lenition of the next following consonant before disappearing itself with the later dropping of unstressed syllables. This may sound rather fanciful but it is suggested as a possible way in which the lenition of the Irish *sléagar* construction might arise, and of course it is completely independent of the incidence of lenition in Hebrew. The latter, indeed, may reflect Aramaic phonetics of the period when Hebrew-speakers adopted Aramaic as their every-day vernacular, rather than the original state of the Canaanite dialects at any earlier time when settlers from the eastern Mediterranean might have reached Ireland in sufficient numbers to establish their language there. A further difficulty is that while an IE stem-form would explain the uninflected *sléagar*-form of a singular noun in Irish, it would not explain the fact that in this construction a genitive plural form is replaced

by the nominative plural; this latter substitution must be taken as being on the analogy of what happens with singular nouns. In Hebrew when the construct form differs from the common form this is due to lack of stress.

It is clear therefore that in this curious parallel between Irish and Hebrew idiom, those parts of the parallel that relate to lenition and the reduction in form of the construct words are purely fortuitous, since each depends on factors peculiar to the language in question. It is only in respect of the word-order and the occurrence or non-occurrence of the definite article that some degree of substratic influence might be claimed. The lenition of intervocalic consonants as a general phonetic principle, leading to initial mutations when it occurs at word junctions within a phrase, is of course something that affects many languages in the Atlanto-Mediterranean area with which we are concerned, but it would extend this paper too much to pursue it here.

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## DISCUSSION

KAYE: These sorts of questions have always interested linguistics, especially the old Italo-Celtic problem for example. Indo-Europeanists today do not talk of an Italo-Celtic branch any more. The strongest proof for Italo-Celtic was the infix *-b-* to form the future tense, which is common to only two Indo-European languages, Latin and Old Irish. I do not know of anything so striking as an infix diffusing or as an areal feature. Yet Indo-Europeanists no longer postulate an Italo-Celtic sub-branch of Indo-European. So even more so, I think that we would need a lot more evidence before postulating a substratum, especially a Phoenician substratum, for Ireland and Britain in general, not to mention Berber.

BYNON: I do not think that we should underestimate the potential importance for our subject of this claim by the Celticists that there is evidence for the existence of some non-Indo-European substratum in Celtic, as well as in the other Western European languages. They must clearly attempt to isolate this substratum and find an origin for it. I do think, however, that they are perhaps expecting a little too much from Berber studies at this stage—we are not really yet in a position to answer their questions. The idea that this pre-Indo-European language of Ireland may have been brought up the West Atlantic seaboard by the megalithic peoples and that it may have belonged to the Hamito-Semitic group is not an unreasonable working hypothesis. Although the Berberists obviously have no way of proving that this language was NOT related to Berber, I think that we have to admit that we are not yet really ready to answer in a positive fashion the questions posed by the author. I hope that this non-committal attitude—due to the fact that our studies are so far behind theirs—will not discourage them from continuing to list those features of Celtic or Western Indo-European which are not explicable in terms of the development of Indo-European and from presenting them to us to look at.





## AN ANCIENT PEOPLE OF THE LIBYAN SAHARA

CHARLES M. DANIELS

This note consists of two sections. The first is a brief introduction to the Garamantes,<sup>1</sup> the second and more important is a collection of epigraphic material from the vicinity of Garama, the Garamantian capital.

The Garamantes were the ancient inhabitants of that area today called Fezzan, the Saharian province of the Libyan Arab Republic. Their capital of Garama has long been argued to lie under the deserted town of Germa in the Wadi el Agial, 105 miles west of Sebha, the modern capital of Fezzan. Recent archaeological work has confirmed this and also shown that the tribe occupied the whole of the cultivable length of the semi-desert el Agial, extending from el Abiad to Tin Abunda (west of Ubari), an area approximately 100 miles in length by between a half and 3 miles in width: a thin valley of cultivation masterfully described by Ptolemy's phrase *kai hē Garamantikē Pháranx*<sup>2</sup> snaking its length, as it does, between the Sand Sea of Ubari and the Hamada of Murzuch, each of which rises to over 500 ft. in places. Recent field-work has shown that the tribe, or its dependencies, also controlled the Wadi Sciatti to the north and the area of Wadi Bergiug-Hofra to the south: the three great lines of Fezzanese oases which run east-west between the barriers of the Hamada el Hamra, the Sand Sea of Ubari and the Sand Sea of Murzuch.

In ancient times the land was better watered, and supported much more animal life, wild and domesticated, as well as considerable agriculture. The tribe, noted by Herodotus as fearless charioteers,<sup>3</sup> was hardy and numerous. Roman contact

<sup>1</sup> There is still little in print that is recent or adequate concerning the Garamantes. A short monograph by the writer, *The Garamantes of Southern Libya*, has recently been published by Oleander Press (Daniels 1970a). It contains accounts of the history, geography and archaeology of the people together with evidence for their social customs. The results of recent Libyan excavations and the writer's excavations and field work are also incorporated, and a bibliography is included. The present note draws on this work, on the introduction to "The Garamantes of Fezzan: excavations on Zinchecra 1965-7" (Daniels 1970b) and a paper summarising the results of recent work and excavations which was given at the University of Libya Conference in Benghazi, 1968 (Daniels 1971). The classic account of the previous expedition to the area is that of Pace, Sergi and Caputo (1951).

<sup>2</sup> Ptolemy, *Geog.*, IV, 6, 3.

<sup>3</sup> Herodotus, *Histories*, IV, 183.

began with the notable campaign of L. Cornelius Balbus in c. 20 B.C.<sup>4</sup> which exacted a sullen submission, broken by aid to the rebel Tacfarinas in A.D. 17-24,<sup>5</sup> and lightning raids upon the coastal provinces, culminating in a violent attack upon Leptis and devastation of its territory during coastal city squabbles of A.D. 69-70.<sup>6</sup> In return the Flavian legate Valerius Festus invaded Garamantian territory and apparently enforced a more enduring peace, opening the land to Roman commerce. The later history of the tribe is less certain: the Severan frontier forts of Ghadames, Gheriat and Bu Ngem suggest continued disturbances although the Garamantes are not specifically mentioned.<sup>7</sup> Instead, their name became a poetic synonym for the southern fastnesses of the Roman world in the manner that the painted Picts symbolized its northern limits. Garama and the Garamantes, however, were still in existence when the first Arab invaders arrived in A.D. 643.<sup>8</sup>

Recent excavation has provided fresh evidence for the history of the people. At the moment of writing the earliest occupation of Garama can be dated to the fourth century B.C. The first stone buildings appear about the first century A.D. and the city remained inhabited until at least the sixth century. Some two and a half miles to the south-west the promontory fortress of Zinchecra shows a longer history. Although not yet dated absolutely, the earliest artefacts recovered there are Neolithic in culture, while the latest pottery is of the first century A.D., after which the lower slopes of the promontory were abandoned to cemeteries. Other habitation sites have been found in the el Agial but none has yet been excavated.

Cemeteries abound. Professor Caputo counted 59,686 graves during the 1933/1934 Expedition to the wadi, though it is now likely that this erred on the side of understatement, by many thousands. Especially imposing stepped cairns occur at points along the escarpment, the Germa Royal Cemetery and Taglit cemetery being two of the most conspicuous. Mud-brick superstructures replace stonework in the cairns of the wadi centre, sometimes taking the shape of 12-15 ft. high pyramids, as at el Hatir and Charaig. The most conspicuous types of funeral furniture are the so-called hand-shaped stele and offering tables, both of which date exclusively to the period of Roman influence (c. 1st century A.D. - Byzantine times). The hands vary in size but are all of sandstone and consist of a palm and four upright fingers, not dissimilar from the later hands of Fatima (see nos. 41-43 below). Occasionally 'pincer-shaped' stele are found, consisting of two 'fingers', and sometimes single stele. The offering tables are rectangular blocks with one large and 5, 7 or more small recesses carved into their top (see nos. 52-55 below).

The most celebrated sepulchral monument in the area is the so-called Germa

<sup>4</sup> Pliny, *N. H.*, V, 34ff.

<sup>5</sup> Tacitus, *Annals*, III, 74, 3.

<sup>6</sup> Tacitus, *Histories*, IV, 50 and Pliny, *N.H.*, V, 34ff.

<sup>7</sup> *The Augustan Histories: Severus XLIII, 3, Inscriptions of Roman Tripolitania* 907, 908, 913-916 and *Libya Antiqua*, Supplements vol. II, 1966, 107-111.

<sup>8</sup> Ibn 'Abd al-Hakam (1948), 63.

mausoleum—more precisely Gasr Uatuat—long thought to be a unique instance of Roman penetration into the Sahara. The *podia* of four similar monuments have been found by the writer within five miles of Garama. All are likely to be the work of Roman artisans and masons sent as a ‘cultural mission’ to the Garamantes following the establishment of peace between the two peoples in Flavian times. This accords well with the first appearance of dressed stone on Zinchecra (in the latest period of all there) and in Garama, where a series of truly impressive stone buildings arose.

Agriculture is attested by the many hundreds of *foggaras* or underground water channels (akin to the *qanats* of Persia) which cross the valley from escarpment to centre for most of its hundred miles length. In one place no less than sixty of these can be counted in a length of 3.5 miles.

The main part of this note comprises the first collection of possible ‘Garamantian’ inscriptions ever to be attempted. Considerable difficulties are at once encountered by anyone attempting this task for it is not even known what language or script the Garamantes used amongst themselves. For that reason all the material available from archaeological sources at the moment of writing is given—including some inscriptions which are not very likely to have been cut by Garamantes. In addition the inscriptions from the cemetery at Taglit are included, for there is (on first consideration) a fair possibility that they are either contemporary with the hands on which they are carved or at least of considerable age. Likewise Aurelius’ name is included for although it is unlikely that the late Garamantes were Greek speaking amongst themselves it may just have been carved by a returning Garamantian mercenary who had taken a Greek name during his service career and risen to the rank of centurion. The material is divided into five groups.

#### GROUP A

This comprises graffiti scratched on to the sides or base of a number of fine red ware dishes recovered from the cemetery of Saniat ben Howedi. This cemetery lies approximately 3000 yards (2.7 kilometres) east of Germa and was excavated by Dr. M. S. Ayoub in 1962-1963.<sup>9</sup> The tombs had originally been covered by mud-brick superstructures in the form of square, stepped *chouchets* which were badly destroyed. However, the graves had apparently survived better and a considerable number of vessels were recovered. These included fine red ware of Italian manufacture, glass bowls and drinking vessels, lamps, flagons and amphorae, all imported, and locally made bowls, plates and cups. Many of the graves had been adorned with tables and hands.

<sup>9</sup> The main report of this excavation is by M. S. Ayoub (1968, 27-51) although shorter, and sometimes conflicting, accounts of the fine red wares, glass and lamps have subsequently been produced by the Southern Governorates Controllery of the Ministry of Tourism and Antiquities, Department of Antiquities.

While the inscriptions could have been added at any time during the life of the vessel it is unlikely that they were scratched by the traders who purchased the dishes from their manufacturers, and a good deal likelier that they were added in the wadi el Agial. One possible alternative is that the plates are loot from the raid on Leptis in A.D. 68/69, but the chance of this is very slight. Two Latin and one Neo-Punic(?) words are recognisable. The name Nimira is known from the pre-Desert area where it occurs at Bir ed-Dreder (*Inscriptions of Roman Tripolitania*, 886) and Ghirza (*I.R.T.* 899). Other forms are Nimmire (Ghirza, *I.R.T.* 898) and possibly N'mrr on a Neo-Punic inscription (*I.R.T.* 11, no. 6). The monogram(?) CA appears three times and seven or eight possible other signs appear, one on four occasions.

All the graffiti are on fine red ware dishes and are now in Sebha Museum. The curved lines above nos. 3 and 4 show the position of the foot ring of the vessel. The lines on nos. 5b and 6b show breaks in the vessel. All the dishes are Italian-made, stamped *terra sigillata* dating c. A.D. 50-80.

- (1) Sebha Mus. Cat. H.34. One symbol scratched on underside.
- (2) Sebha Mus. Cat. H.38. One symbol scratched on underside.
- (3) Sebha Mus. Cat. H.40. (a) Symbol(?) scratched within basal ring on underside. (b) One symbol and one possible symbol scratched on underside, which could be a chance marking. (c) Neo-Punic(?) inscription scratched on underside. (d) Latin inscription (CURA ?) scratched on underside.
- (4) Sebha Mus. Cat. H.83. Two symbols scratched on underside.
- (5) Sebha Mus. Cat. H.84. (a) Latin inscription NIMIRA (proper name?) scratched on side, above carination, and again very faintly as NIM[ on other side (not shown). (b) One symbol on underside (vessel broken and mended).
- (6) Sebha Mus. Cat. H.85. (a) Incomplete and very faint Latin inscription NIMIR[ scratched on wall (not shown). (b) Two symbols, one incomplete, scratched on underside.
- (7) Sebha Mus. Cat. H.86. One symbol scratched on underside within basal ring.

#### GROUP B

This group consists of symbols cut or painted on dark grey amphorae from the same cemetery. Most have been transported to Sebha Museum but a few are still at Germa. The Ayoub Reports referred to in footnote 9 place the amphorae in groups and it may appear that they can be dated from other material in the groups. However, from a study of the successive reports, my notes made at Saniat Gebril during part of the excavation and discussions with Dr. J. Hayes, who saw the material shortly after the excavation, I feel that it would be rash to treat these 'groups' as sealed archaeological grave groups or attempt to date the amphorae from their attributed contexts.

The signs can be divided into three categories. The first consists of seven signs which were clearly cut into the wet clay of the amphora before firing: 8, 9, 17, 18, 20, 24 and ? 29. This group is almost certainly NOT Garamantian, as the amphorae were almost certainly imported, and the signs are given for comparison with the others. The second category is of twelve signs clearly cut after firing and from the fresh condition of many almost certainly added in the wadi el Agial: nos. 10, 11, 12, 13, 15, 16, 19, 21, 22, 23, 26, 27 and 28. The third category is of seven signs applied in red paint (haematite) which has been found extensively used in Garamantian decoration and colouring. These symbols are often faint and weathered or rain-washed.<sup>10</sup> One is added over the Neo-Punic(?) inscription no. 24: numbers of the group are: 11, 12, 14, 20, 23, 24 and 25.

- (8) Sebha Mus. Cat. A.8. One symbol inscribed before firing.
- (9) Sebha Mus. Cat. A.10. One symbol inscribed before firing.
- (10) Sebha Mus. Cat. A.13. One symbol cut after firing.
- (11) Sebha Mus. Cat. A.15. (a) Three symbols cut after firing. (b) One symbol applied in red paint.
- (12) Sebha Mus. Cat. A.21. (a) One symbol cut after firing. (b) One symbol applied in red paint.
- (13) Sebha Mus. Cat. A.22. One symbol cut after firing.
- (14) Sebha Mus. Cat. A.37. One symbol applied in red paint.
- (15) Sebha Mus. Cat. A.41. One symbol cut after firing.
- (16) Sebha Mus. Cat. A.42. One symbol cut after firing.
- (17) Sebha Mus. Cat. A.47. One symbol inscribed before firing.
- (18) Sebha Mus. Cat. A.49. One symbol inscribed before firing.
- (19) Sebha Mus. Cat. A.50. One symbol cut after firing.
- (20) Sebha Mus. Cat. A.55. (a) One symbol inscribed before firing. (b) One symbol applied in red paint.
- (21) Sebha Mus. Cat. A.62. One symbol cut after firing (looks rather recent).
- (22) Sebha Mus. Cat. A.63. One symbol cut after firing.
- (23) Sebha Mus. Cat. A.64. (a) Three symbols cut after firing. (b) One symbol applied in red paint.
- (24) Sebha Mus. Cat. A.66. (a) One symbol inscribed before firing (Neo-Punic?). (b) One symbol applied in red paint.
- (25) Sebha Mus. Cat. A.67. One symbol applied in red paint.
- (26) Still at Germa Rest House, no number. One symbol cut after firing.
- (27) Still at Germa Rest House, no number. One symbol cut after firing.

<sup>10</sup> On my return to Tripoli from Fezzan in 1969 Signor M. Fabbri presented me with his published list of these symbols which shows serious divergencies from mine. While I cannot vouch for the complete accuracy or correctness of the painted signs I give here (which were not copied by my draughtsman on that occasion) I can vouch for my versions of the inscribed signs, against Signor Fabbri's (Corrain, Fabbri and Zampini, 1967, 3ff.).

- (28) Still at Germa Rest House, no number. One symbol cut after firing.
- (29) Still at Germa Rest House, no number. One symbol inscribed before firing(?).

#### GROUP C

This group consists of all the fragments of amphorae from other sites which display letters or fragmentary signs. Most come from the vicinity of Germa, although one was recovered at Charaig cemetery.

The Royal Cemetery<sup>11</sup> is a large and imposing cemetery of stone-built, stepped *chouchets* which lies some 5,000 yards (4.5 kilometres) south-east of Germa. It is probably the most imposing, as well as one of the most important of the cemeteries so far known in the el Agial, although it has suffered considerable damage from tomb robbers.

Charaig<sup>12</sup> cemetery is approximately 8.25 miles (13 kilometres) east of Germa at the foot of an isolated bluff on the south side of the wadi. The tombs are small, square, mud-brick pyramids covering shaft graves, but all are now in a very damaged condition. The adjacent slopes of the escarpment are covered with late first to fourth or even fifth century coarse and fine red wares. The amphora sherd in question is undated.

The Germa escarpment area is the wide 'bay' in the southern escarpment wall of the wadi south of Germa where spreads of cairn-cemeteries lie either at the escarpment foot or on its lower slopes. These range in date from the late first to the fourth century A.D.

Zinhecra<sup>13</sup> is the early fortified promontory site 4,000 yards (3.6 kilometres) to the south-west of Germa. It is covered by some hundreds of habitation sites ranging from rough shelters to well-built huts, which occupy 10-20 acres (4-8 hectares) of the promontory's slopes and flat summit. The fragment of amphora in question was found in the vicinity of some late occupation sites close to the top of the spur.

Germa<sup>14</sup> is the mediaeval mud-brick city which now covers the western portion of the Garamantian capital of Garama. Excavations here have been carried out since 1961 and have produced a series of stone buildings ranging from the second to the fourth centuries A.D. in date. Saniat Gebril is the now unencumbered eastern area of Germa. Trial trenching has produced mud-brick buildings of late first to early third century A.D. date and suggests that the area was intensely occupied during that period. Our present state of knowledge suggests that the total area of Germa must have been about 50-55 acres (20-22 hectares), part of which was intensi-

<sup>11</sup> Professor Caputo's *Necropoli Occidentale* and *Monumentale* (Pace, Sergi and Caputo, 1951, 292-360) and Ayoub (1968), 11-22.

<sup>12</sup> Pace, Sergi and Caputo (1951), 363-373.

<sup>13</sup> Daniels (1970b).

<sup>14</sup> Ayoub (1968), 23-39.

vely occupied while other parts were much more lightly occupied. The site seems to have been inhabited from the fourth century B.C. to the Arab conquest and probably later.

- (30) Fragment of a reddish-brown amphora wall excavated from Royal Cemetery Tomb 5 in 1963. Parts of four letters?
- (31) Fragment of a brownish amphora handle from Saniat Gebril, surface find 1965 (no. 302). One symbol.
- (32) Fragment of an amphora of uncertain origin, 1965. Part of one sign?
- (33) Fragment of an amphora of uncertain origin, 1965. Part of one sign?
- (34) Fragment of an amphora of uncertain origin, 1965. Part of one sign?
- (35) Fragment of an amphora of uncertain origin, 1965. Part of one sign?
- (36) Fragment of an amphora of uncertain origin, 1965. Part of one sign?
- (37) Fragment of a reddish-brown amphora neck, Zinchecra surface find in 1965 on the upper slopes of the spur (no. 545). Parts of two letters.
- (38) Fragment of a dark grey amphora, similar to nos. 8-29, recovered spoil heap Germa 1967 (no. 1001). One symbol.
- (39) Fragment of a dark grey amphora wall, surface find Charaig cemetery 1969, (no. 1976). Part of one symbol.
- (40) Fragment of a dark liver-coloured amphora, Germa escarpment find 1967 (no. 1325). Part of one symbol.

#### GROUP D

This consists of 'inscriptions' or groups of symbols carved on to hand-shaped stele, offering tables or smooth rock surfaces at the late Roman (Byzantine?) cemetery of Taglit.<sup>15</sup> The site lies 10.5 miles (33.5 kilometres) west of Germa on the western slope of one of the more pronounced promontories of the wadi escarpment and consists for the most part of large stepped *chouchets* packed together in a tight group. The hands and tables form a conspicuous part of the cemetery. While it is not certain that these inscriptions are actually contemporary with the objects on which they are inscribed they are consistent in that they show the same degree of weathering and damage as those objects, and do not reveal any difference of patination. Inscriptions 41-43 have apparently been arranged on the stele with some care and no. 49 is on a stone stele *in situ* which must have been inscribed before the stele became buried to its present level. No. 48 is unique. Nos. 46, 47, 50 and 51 are on natural stone and possibly not in the same category as the rest.<sup>16</sup>

These inscriptions were copied by an architectural student who accompanied me

<sup>15</sup> Pace, Sergi and Caputo (1951), 381-384.

<sup>16</sup> Compare the inscribed stele shown by Reygasse (1956), 49f., fig. 53.



to the wadi in 1959. As can be seen by comparing the drawing and photograph of no. 41 the reproduction is pretty accurate, although the inscriptions have not been checked since they were drawn and alternative interpretations of some symbols may be possible.

- (41) Inscriptions carved (a) on the front and (b) on the side of hand-shaped stele no. H.59.
- (42) Inscription carved on the face of hand-shaped stele no. H.73.
- (43) Inscription carved on the face of hand-shaped stele no. H.116.
- (44) Inscription carved on fragment of a single stele, no. H.6.
- (45) Inscription carved on fragment of a single stele, no. H.42.
- (46) Inscription carved on unworked piece of stone, inscription no. 6.
- (47) Inscription carved on unworked piece of stone, inscription no. 3.
- (48) Inscription carved on dressed gravestone? No. SP.3.
- (49) Inscription carved on fragment of hand-shaped stele still *in situ* at grave, no. H.8.
- (50) Inscription carved on small fragment of stone, no. SP.5.
- (51) Inscription carved on small fragment of stone, no. SP.6.
- (52) Inscription cut on to top surface of offering table, no. T.16.
- (53) Inscription cut on to top surface of offering table, no. T.26.
- (54) Inscription cut on to top surface of offering table, no. T.25.
- (55) Symbol cut on to side of offering table, no. T.45. (Camel brand?).

#### GROUP E

This group consists of the remaining inscriptions carved on objects from various sites, for the location of which see section C above.

- (56) A single inscribed hand from Charaig cemetery (no number). However, the symbols are cruder and noticeably different from those noted at Taglit and the question of date remains open.
- (57) The symbols carved on two sides of a terra-cotta loom weight of the 3rd century A.D. This was excavated in a room of house no. 4 at Saniat Gebril in 1965. The symbols may not be Garamantian at all as not dissimilar signs have been found on loom weights as far away as Southern Gaul. Side b is damaged and the precise shape of the symbol unclear.
- (58) A possible inscription carved on a fragment of painted wall plaster recovered from the spoil heap in the vicinity of building I at Germa.<sup>17</sup> The wall plaster would not be out of place on a classical Roman site, and in this is different from the much thinner and sandier wash found employed in the buildings

<sup>17</sup> Ayoub (1968), Building A.





FIG. 1  
Fine red ware dish with graffiti:  
Saniat ben Howedi cemetery.  
Catalogue No. 3 (Group A).



FIG. 2  
Amphora with incised sign:  
Saniat ben Howedi cemetery.  
Catalogue No. 12 (Group B).



FIG. 3  
Hand with inscription:  
Taglit cemetery.  
Catalogue No. 41  
(Group D).



FIG. 4  
*Tifinagh* inscription from  
the wadi, unknown  
provenance and date.  
Sebha Museum.  
This inscription is typical  
of the many unrecorded  
*tifinagh* texts from the  
wadi el Agial.

at Saniat Gebril. The pecking of the inscription is somewhat rough and the characters not altogether certain.

- (59) A Greek inscription carved on the top surface of Zincheera promontory towards its eastern end.<sup>18</sup> It apparently records the name of the centurion Aurelius, but it is not altogether certain that the name and the 7 are by the same hand. Mr. P. M. Fraser has commented that the inscription appears to be of the third or fourth century, although even that is uncertain. If the 7 is a centurial sign then the fourth century is the latest date that it could be. As already said we may have a graffito scratched by a visiting Greek, or the name of a Garamanian who had risen to rank in the eastern army and then returned to his home.

In addition to the items listed above there is a considerable number of *tifnagh* inscriptions from the wadi el Agial and some of these have already been collected by the writer and Lady Brogan. They are not included here, however, as the antiquity of no single one of them is certain and in several cases it is very unlikely. In this they do not fall into the same category as those which have been listed. On the other hand, it would be well worth while collecting ALL the *tifnagh* inscriptions from the wadi into a single working corpus, for until this is done and studied we will never know the range and variety of material represented nor whether any of it is GARAMANTIAN as opposed to TWAREG or contains other information of interest or importance.

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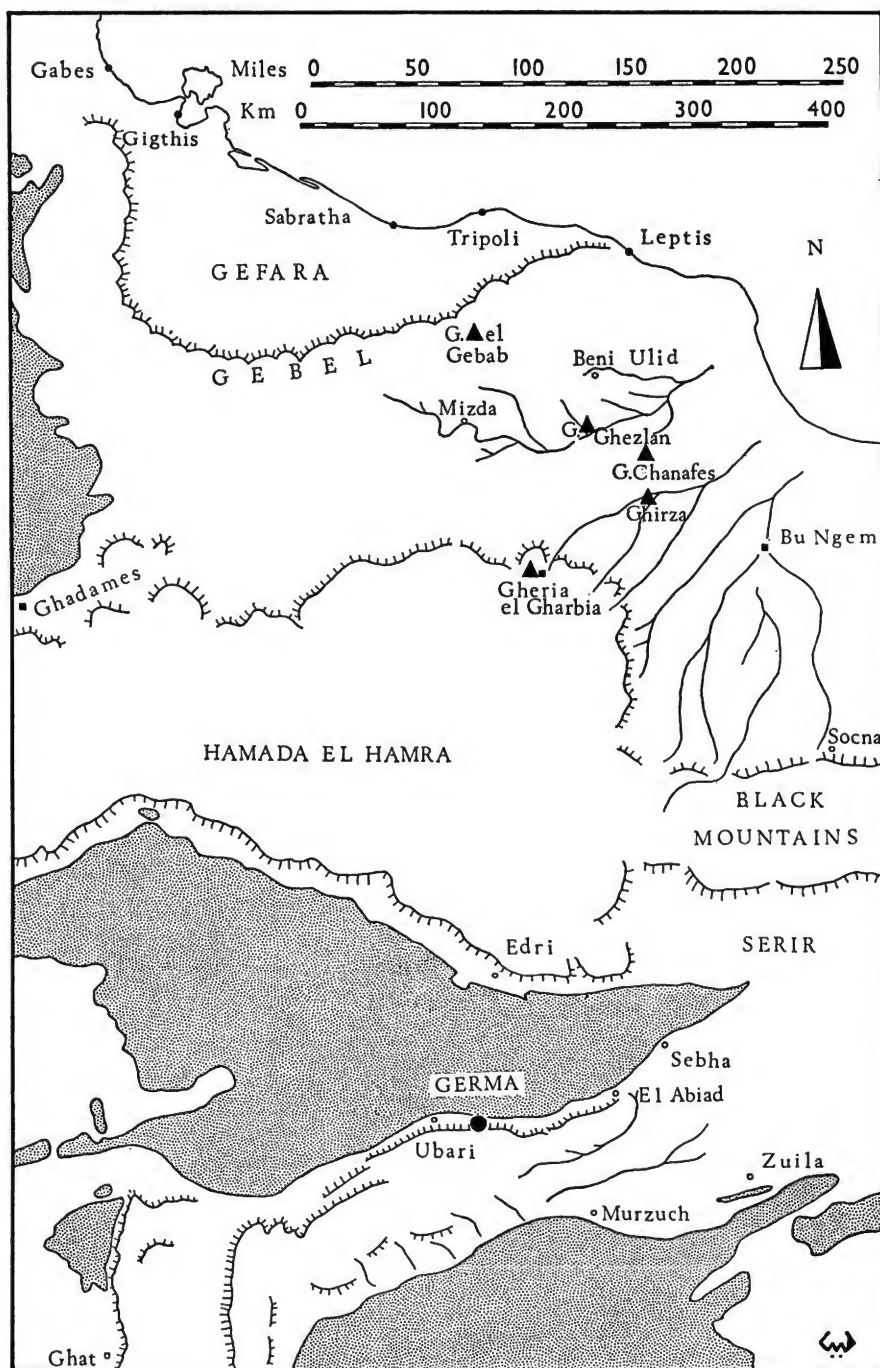
## DISCUSSION

BYNON: This paper brings us early linguistic data from North Africa in the form of inscriptions from the ancient capital of the Garamantes in the Fezzan. The most important from our point of view are those of group D from late Roman hand-shaped stele and offering tables, some of them found *in situ*. I would like to underline the great importance for Libyco-Berber studies of this possibility, that we may eventually have quite a considerable corpus of written material, fairly well dated and localized and associated with well defined archaeological strata. Mr. Daniels talks about 60,000 tombs, presumably about 59,000 of them unexcavated? If only a moderate number of them contain written material, then we may eventually have a very reasonable corpus to work with. I do not know whether there will be as much discussion as the author hopes of the actual contents of the linguistic material presented, although this has the undoubted attraction of being undeciphered and hence all the mystery of the unknown ... which is always a great temptation. When I say unknown of course I do not mean COMPLETELY unknown—any Berberist can look at them and most, if not all, of the signs will be already familiar to him and phonetic values can be attributed to many with really very little risk of error. But the bigger the corpus the better the chance of our eventually reading them.

DANIELS: As far as the Fezzanese material of Garamantian origin is concerned, I think there is a chance, but there are still many uncertainties. I have been working on settlement sites, habitation sites, and not in cemeteries up to the moment. This is purely because the previous work that had been done by Caputo in the thirties had been done on cemeteries. So I started on the habitation sites, the place that required work because nothing had been done, and little has turned up: really only nos. 31-36 and 38 of what I have listed have come from habitation sites. But I hope shortly to turn to cemeteries and, as you can see there are two lots of material, and I think we probably will get it accumulating although the individual inscriptions may be very short. If one takes it in two parts: nos. 41 to 56 (this is a group of inscriptions inscribed on stones) I think the inscriptions are contemporary with the cemetery. This is not absolutely certain, but there is always the possibility of more material of this sort turning up and for instance if we find stele that have fallen down and been covered and we find them in archaeological strata then we can be sure of their date. The other, which unfortunately is the group of smaller, shorter, and more fragmentary inscriptions, is the material that occurs actually inscribed on pottery of one sort or another, and as you see there is a fair range of languages represented, a fair range of alphabets: nos. 1 to 29 and no. 30—and in fact no. 30, fragmentary though it is, is the only one that suggests that there has been at some time an inscription of some slight length in the Tifinagh alphabet. But I think that from the cemeteries we may well get some stuff. A German scholar has been excavating something like

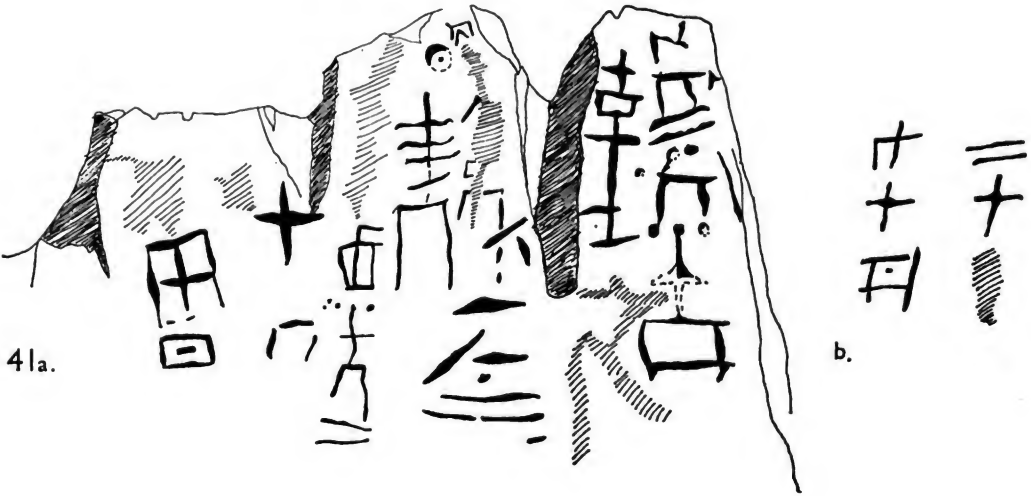
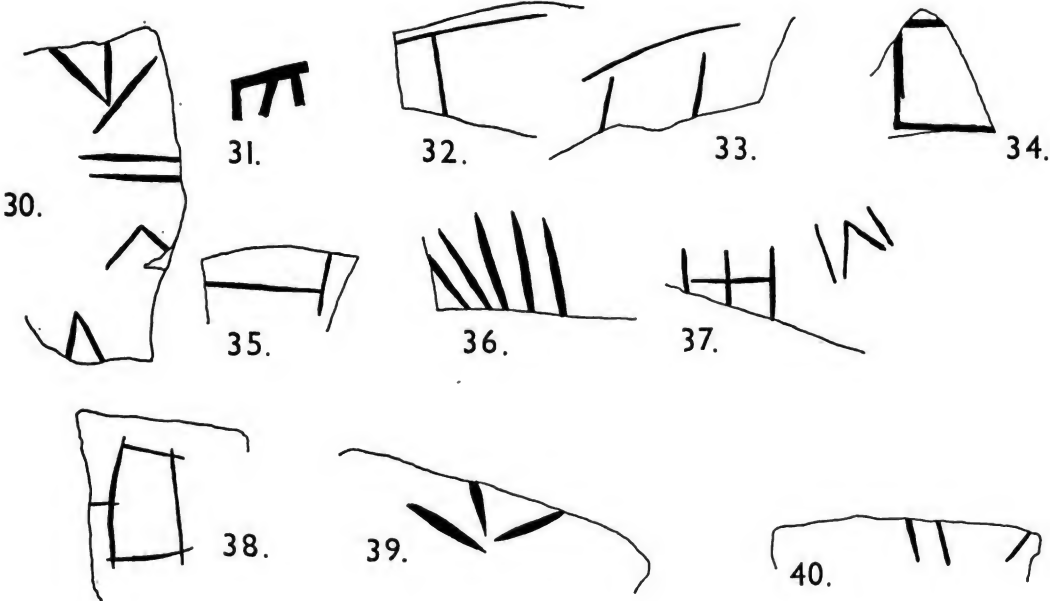
forty miles from where I have been working. His results are not yet published. He has been digging graves and I have heard that he found a leather shroud which had got a tfinagh inscription of some length on it. But I have not seen it and know none of the details. But if this sort of thing is going to turn up, and if we can get some unrobbed graves—unfortunately I think 49.5 of the 50 thousand have been robbed rather than are there awaiting the archaeologist .... So the odds are not as good as they might appear to be at first glance.

BYNON: Mr. Daniels says that these inscriptions are short. But they promise from the contexts to be interesting because they may, especially the ones on the bowls etc., be of a different NATURE from the typical Libyan inscriptions that we are used to from tombs, which have only given us personal names so far, and have not told us very much about the morphology or the syntax of the language.

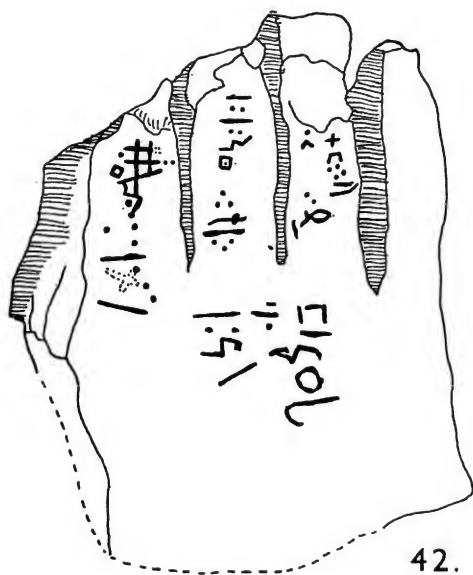


LIBYA : Map showing the coastal cities, the frontier forts (square symbols), pre-desert sites which have produced tiffinagh inscriptions (triangular symbols) and the Garamantian area (Edri, Ubari-el Abiad and Murzuch-Zuila). Modern sites are represented by open circles.





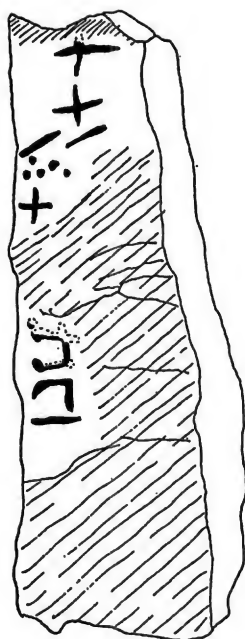




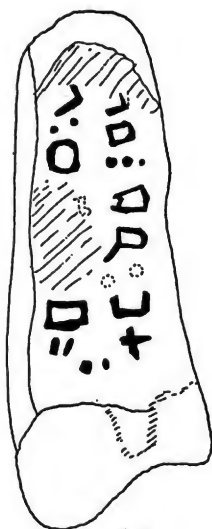
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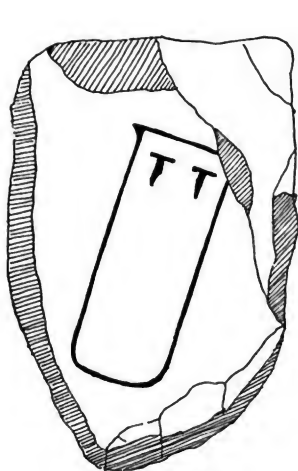
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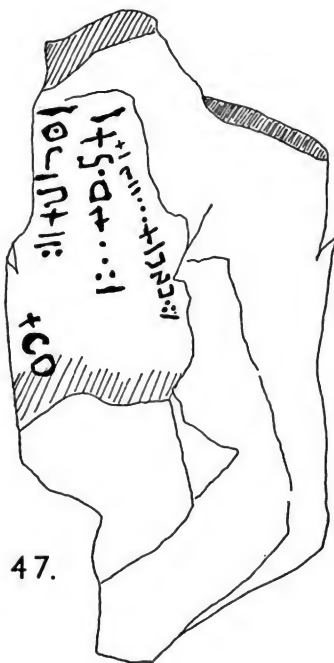
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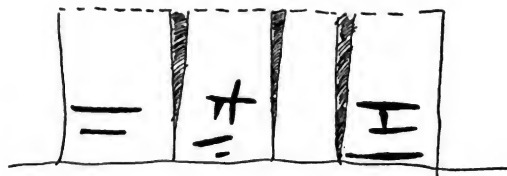




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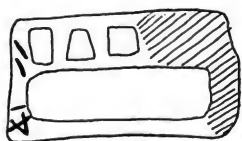
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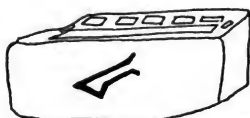
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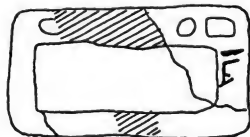
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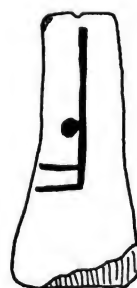
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57a.



b.



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59. ΑΟΥΡΕΛΙΟΥΣ 7



## INSCRIPTIONS IN THE LIBYAN ALPHABET FROM TRIPOLITANIA, AND SOME NOTES ON THE TRIBES OF THE REGION

OLWEN BROGAN

### I. TRIPOLITANIA, LIBYPHOENICIANS AND LIBYANS

The discovery in the Tripolitanian pre-desert of inscriptions in the Libyan alphabet dated to the late Roman period raises the questions of who could have carved them, and consequently, of the nature of the population of Tripolitania in antiquity.<sup>1</sup>

The area with which we are concerned (map on p. 260) consists of (1) the coastal plain of western Libya, the Gefara, which extends into Tunisia. It is bounded on the west, south and east by (2) the Gebel, the hill country, which runs in a great semi-circle from the neighbourhood of Gabes (ancient Tacapae) in Tunisia to the Tripolitanian coast just west of Homs and Lepcis Magna. (3) East of Homs the coastal plain starts anew, at first as a narrow strip, continues to Misurata and then round the Greater Syrtis (the Gulf of Sidra) in an unbroken sweep until it reaches the foot of the Cyrenaican Gebel. South of Syrtica it rapidly merges into the desert. (4) The southward and eastward slopes of the Tripolitanian Gebel drain into two major wadi systems, the Wadi Sofeggin, which flows into the marshes of Tauorga on the west of the Greater Syrtis and the Wadi Tareglat, which reaches the sea as the Wadi Caam (Cinyps of Herodotus) seventeen kilometres east of Lepcis. South of the Sofeggin basin are, in turn, the Wadi Zemzem, flowing north-eastward from the Hamada el-Hamra to the southern end of the Tauorga marshes, and the Wadi Bei el-Kebir, which flows north parallel to the Hamada, receiving the drainage of its eastern slopes, and then north-east to the Gulf of Sidra.

Along the Gebel and in many of the wadis further south there was a settled farming population in the early centuries A.D. which was strongly Punicized in the north and which still retained traces of Punic influence far down into what is commonly called the pre-desert zone. The better lands of the Gebel and coast were largely occupied by the estates of the wealthier citizens of the *Emporia*, the old Phoenician foundations of Sabratha, Oea (Tripoli) and Lepcis Magna. The Libyans near the

<sup>1</sup> For the history and settlement of Punic and Roman Tripolitania, see Haynes (1956); Ward Perkins and Goodchild (1949); Goodchild (1950a and 1951); Oates (1953 and 1954); Brogan (1964); Di Vita (1964); Brogan and Smith (1957); Merighi (1940) (with very full bibliography).

*Emporia* were bound to feel the impact of the Punic and, later, the Roman civilizations. Lepcis itself had from early days been in close relationship with the Libyans round about, and there was considerable intermarriage, so that a Libyphoenician population had grown up, mixed in blood, but Punic in outlook and culture, as literary and epigraphic records make plain (Herodotus V, 42, 3; Sallust, *Bell. Iugurth.* LXXVIII, 4; Gsell 1913-1929, IV, 173, 493-494; Reynolds and Ward Perkins 1952, 11-13).

Inscriptions and *ostraka* with Neo-Punic texts of the first and perhaps the second centuries A.D. have also been found in the Gebel and pre-desert (Levi Della Vida 1951, 1964a and 1964b). In the Wadi el-Ageram (Wadi Zemzem basin) the Punic name Annobal still occurs on two tombs which are probably to be dated to the mid-third century (Reynolds 1955, 141-142). The inscriptions are in the Latin alphabet, but the language was shown by the late Professor Levi Della Vida to be a debased form of Punic (Levi Della Vida 1963, 1965). There are yet other inscriptions of the late Roman period in the Latin alphabet, however, which Levi Della Vida could not construe as Punic and which are not in Latin. They may, as Goodchild suggested, be in a Libyan language (Goodchild 1950b).

Throughout the period the names on all the pre-desert inscriptions are overwhelmingly Libyan, such as Nimira, Nasif, Masukan, Chullam, though there are Punic names too and from the second century some Latin names appear, reflecting the process of Romanization (Reynolds and Ward Perkins 1952, nos. 886, 898-900; Levi Della Vida 1963). The chiefs were rich enough to afford grand tombs and pay masons to come and decorate them with sculpture and inscriptions (e.g. Haynes 1956, plates 28-31). Christianity was adopted by some at least of the inhabitants of the Wadi Sofeggin area in the fifth and sixth centuries (Ward Perkins and Goodchild 1953, 50-56), but there is no sign of this in the next major wadi zone to the south, the Wadi Zemzem, except for sherds of fifth-century lamps bearing the Christian cross, which may simply have come in in the course of trade. Instead, Dr. David Smith and the writer found at Ghirza what turned out to be a pagan temple.

## II. THE LIBYAN ALPHABET

In the debris of the temple there were about twenty small votive altars of pagan type, on three of which were inscriptions in yet another script,<sup>2</sup> the Libyan alphabet which has been found and read in Tunisia and Algeria (Reynolds, Brogan and Smith 1958; Levi Della Vida 1965, 61-62; Chabot 1940). We have been unable to read these and examples are shown here in the hope that there may be somebody in this Colloquium who may be able to help in their decipherment. Besides these little altars (figs. 2 and 3) and the inscribed rim of a bowl (fig. 1) from the temple,

<sup>2</sup> To be published in the *Ghirza Report*.

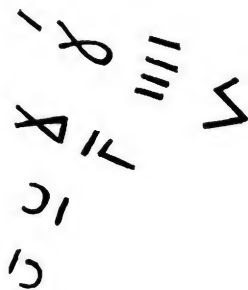


fig.1 GHIRZA bowl

b    √    ≡    EI    +    ∪    )    ]    ∞    |    >    /    <    □    =

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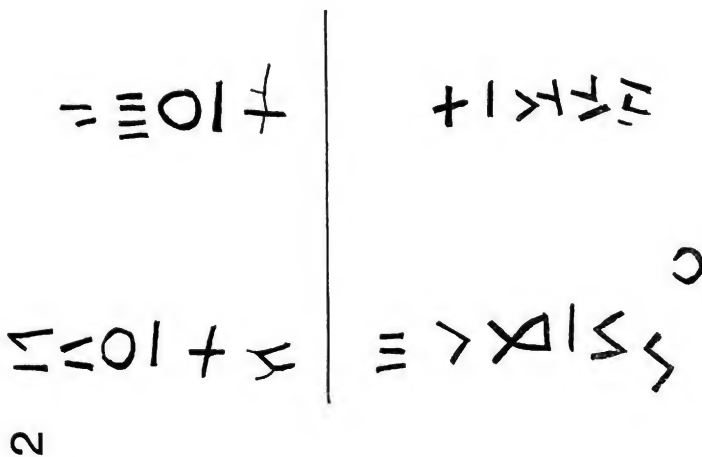
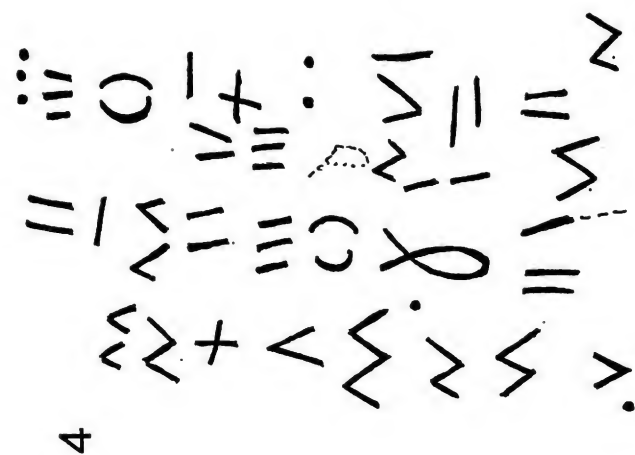
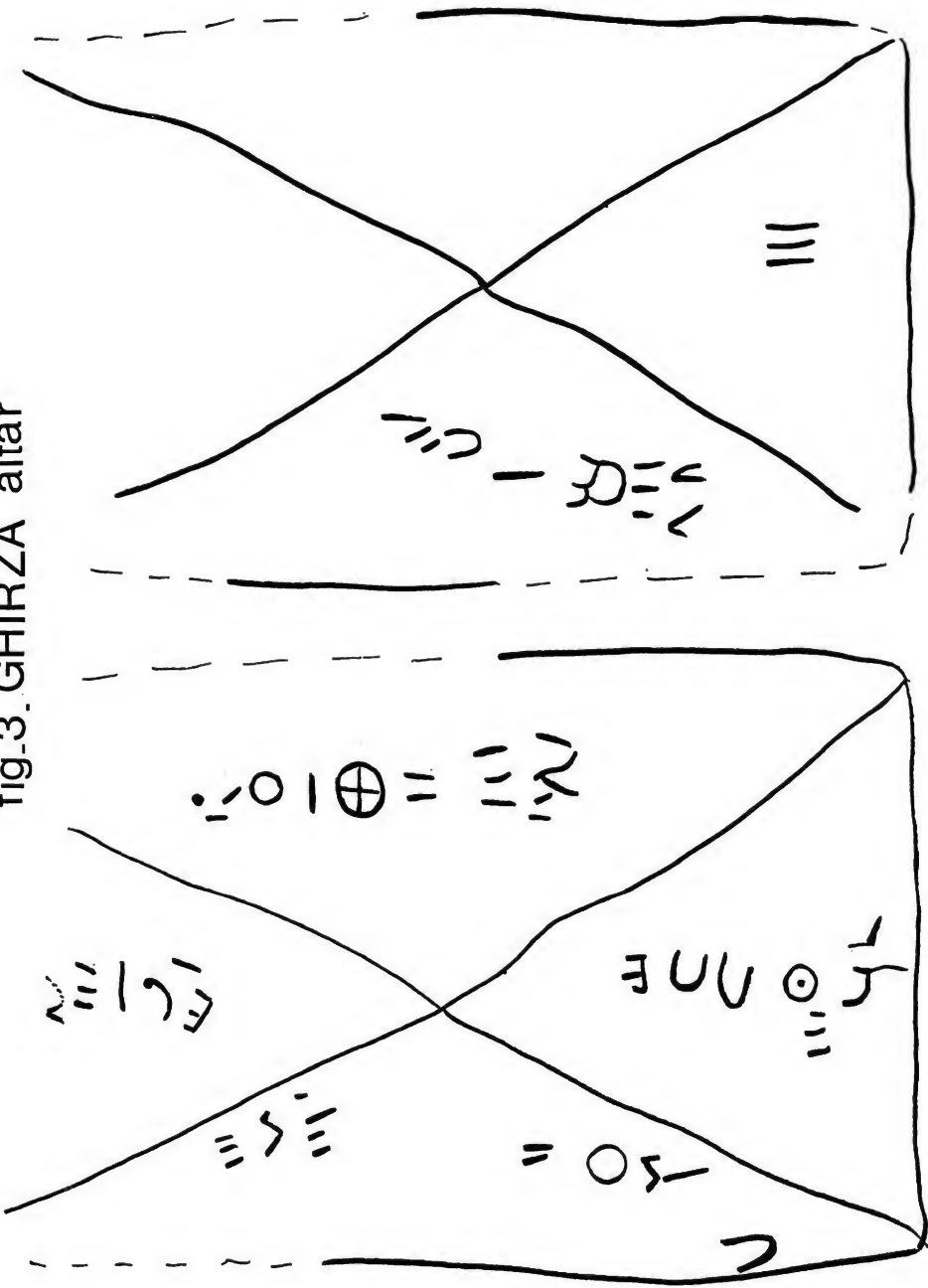


fig.2.GHIRZA altars



fig.3. GHIRZA altar



$$= + \cdot \equiv \cdot \cdot$$
$$1 = 11 \pm 1 \{ -1 - 1 \} =$$
$$I > I - \alpha$$
$$1 + 118 =$$
 $\cdot \text{I} \Pi \exists / \text{F} \text{I} \{ = \cup \parallel =$ 

$\frac{1}{t} + \frac{1}{t+1} + \frac{1}{t+2} + \dots + \frac{1}{t+n}$

14

a 101172

○

1+x1E1} 31 /  
p=kon?L

$\Gamma_{p \geq 1} \langle \phi \rangle_L$

$$- = || \text{ } \rangle$$
$$15 \equiv 15$$

31=

$$1 + \lambda |E| z^{-1} = \sum_{i=0}^{\infty} \lambda^i \sigma_i$$
$$115113 = 511$$

Q

• **oo**

u + 113

fig.4\_GHIRZA house

21 0 I C :: > . : > 0 I

20 11 b + > I 1 + >

13 x > 1 + ^ ≡ 1 ∞ + 1 < > , >

12 1 ≡ 1 . 1 = x  
 12 1 1 > 11 I ≡ =

11 1 + + 1 . + 1 < 1

10 0 v n 0 ≡ > ≡ ≡ 0 x 1 ≡

fig. 5. GHIRZA

24  
 11X + I I ≡ :: { O > 1 } 1 ÷ < ⊖ O 1 ? = H

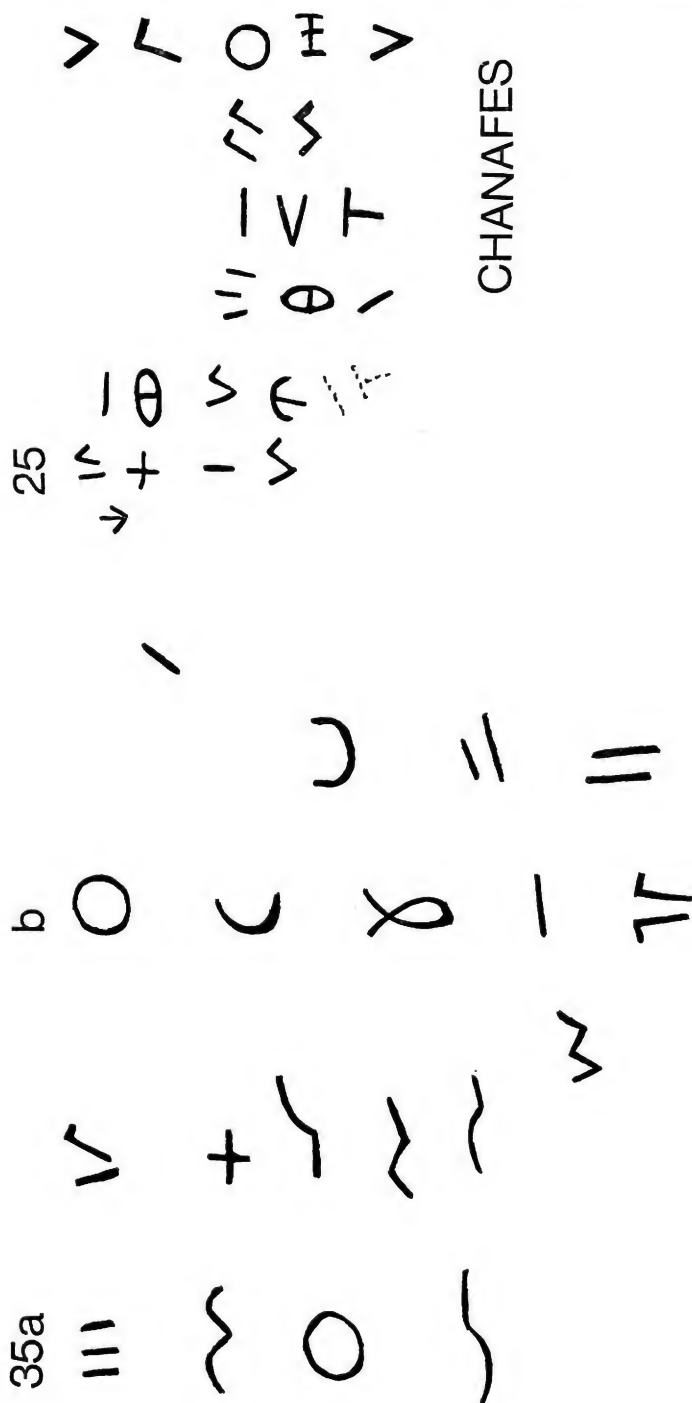
29  
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+ I + I I

34  
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fig\_6\_ - 24-GHIRZA  
 29-WADI GHESLAN  
 34-GHERIA-EL GHARBIA



we have found more than thirty Libyan (or *tifinagh* ?) inscriptions in the pre-desert (e.g. figs. 5-7), and others have since been found and brought to Tripoli (Di Vita 1964, 141-142).<sup>3</sup> The inscriptions on the altars show a fairly orderly arrangement. They are incised with some care and the letters are about one centimetre in height. The temple was destroyed but later partly rebuilt as a Berber house, which was in occupation in the tenth century. Scratched on the plaster of its walls were more inscriptions in the Libyan alphabet (fig. 4). Other inscriptions of uncertain date and often only lightly scratched, found at Ghirza and elsewhere, occur on door posts of Roman houses or on Roman tombs. One, no. 34, is cut in the plaster of a Roman cistern.

The Libyan alphabet originated, so far as is known, in the old Numidian kingdom on the borders of Algeria and Tunisia and in its earliest form (seen on the inscription from Dougga, second century B.C.) precedes Neo-Punic (Chabot 1940, no. 1). It can be assumed that it was brought into Tripolitania from the west, but there is as yet no trace of its passage except a small group of inscriptions from southern Tunisia east of the Shott el-Jerid noted by Chabot who was unable, however, to translate them (Chabot 1940, section III).<sup>4</sup> In the Roman period the Tripolitanian *hinterland*, where literacy must always have been minimal, produces inscriptions in Neo-Punic, Latin, and Latino-Punic shading into Latino-Libyan. At an unknown date there comes the Libyan alphabet which was to remain in use among the Berber tribes hereabouts in the Middle Ages.

Who were its original carriers? Inland Tripolitania had been for a time part of Massinissa's kingdom and this association with Numidia was continued under the Romans who treated the whole frontier area from Lambaesis to Syrtica as a unit. The Libyan script is generally found in the remoter parts of Algeria and Tunisia in the Roman period, away from the military centres. It must have spread to Tripolitania through the movement of obscure persons, possibly camp-followers or persons doing forced labour for the army. Or one thinks of the various Gaetulian tribes who moved about in the south of the provinces, or even of the Numidians themselves, especially of those who passed through southern Tunisia to maintain communications with the Garamantes during the wars against the Romans in the time of Augustus and Tiberius. The desert lands have always seen much movement to and fro so that despite the distances it is not surprising to find that the Libyan alphabet reached not only Tripolitania but also, at a period not yet known, the country of the Garamantes, modern Fezzan.

<sup>3</sup> The blocks on which the inscriptions had been carved were found re-used in a building which, it is suggested, may date from the third century, but with the evidence at present available it cannot be accepted with certainty that they were not cut after the erection of the building.

<sup>4</sup> One of them was found 4 km. from Uazzen, on the Tripolitanian frontier.

## III. THE INHABITANTS OF TRIPOLITANIA AND THEIR NEIGHBOURS

It may perhaps be worth while to take a look at some of the tribal elements in Tripolitania among some of whom the Libyan alphabet came to be used.

The task of sorting out the ethnography of Roman Africa has been attempted by many scholars with limited success because of the confusion of the ancient sources, though linguistics and epigraphy are gradually helping to elucidate some corners of the large subject. Jehan Desanges in 1962 published a valuable systematic account of the tribes known in classical antiquity which all students in this field will be dependent on for some time to come (Desanges 1962; Bates 1914; Gsell 1913-1929, V, chapter 2; Tissot 1884, I, 385-437; Kwapong 1959).

There is a bewildering number of tribal names. A few major groups cover wide expanses of territory. The Gaetuli ranged all along the desert fringe; in the west were the Mauri, the Moors, but the appellation 'Mauri' is by the time of the later empire used for all North Africans other than Roman provincials. In another category are minor tribes of which all knowledge other than their names has too often been completely lost.

*Numidae* (Gsell 1913-1929, V, 105-106)

Herodotus divides the Libyans into two groups, the agricultural population and the shepherds (the Nomades) of which Numidae is the Latin form. The Numidae of the Second Punic War are essentially the tribes of the Masaesyli and the Massyli, the subjects respectively of Syphax and Masinissa, and it appears that the influence of the Massyli extended into the country east of Lake Tritonis (Shott el-Jerid). The name Numidia thereafter became attached to the kingdom of Masinissa which finally included the *hinterland* of Tripolitania; it lived on into the Roman empire and in the early third century was used officially to denote the province of Numidia, of which the frontier area of Tripolitania was a part. As a tribal name it disappears in the late Empire.

*Gaetuli* (Gsell 1913-1929, V, 109 ff.)

The nomadic tribes along the fringes of Roman Africa, within and without, are generally called Gaetuli by Roman writers. The name first appears towards the end of the second century B.C. and is used loosely for the peoples outside the old Masaesylian and Massylian kingdoms and outside former Carthaginian territory. Like so many names which came into general use, it may have arisen as that of one tribe and then been applied to those of a whole region, though this is conjecture. Apuleius (*Apologia*, XII, 5) speaks of his birthplace, Madauros, as close to the border between the Numidians and the Gaetulians, which shows that in the second century A.D.

many Gaetuli lived far within the Roman provinces.<sup>5</sup> The Peutinger Table puts a tribe, the Bagigetuli, along the southern border of Tripolitania.<sup>6</sup> Eastwards, Gaetulia reached to the coastlands of the Greater Syrtis (Virgil, *Aen.*, V, 192; Florus, II, 31; Peutinger Table). This shows the very general way in which the name is used, for the Syrtic coast had long been occupied by the tribes of Macae, Nasamones and Psylli, who were all three still there in the Roman period.

*Mazices* (Gsell 1913-1929, V, 116-120; Desanges 1962, 63, 112)

The wide range of the Mazices resembles that of the Gaetuli. They are of special interest because their name reappears in that of Mazigh, the legendary ancestor of the Beranes, one of the two branches of the Berbers, or sometimes, the ancestor of the whole Berber people (see p. 285). A number of Berber tribes still call themselves the Amazigh (pl. Imazighen), the 'noble ones', and their language Tamazight.

The Mazices may be the same as the Mazyes of Hecataeus or the Maxyes of Herodotus. They are mentioned throughout Roman times from Morocco to the Egyptian border. A fourth-century writer states that they inhabit the lands south of Africa, that is south of Tunisia and Tripolitania, and late imperial sources refer to raids made by them into Tripolitania. They appear on inscriptions, including one from Lambaesis, and in fifth-century Numidia there were two *episcopi Mazacenses*.

The personal name, Mazic, with variants, appears on numerous Latin inscriptions, and the form MSK is found on Libyan inscriptions (Chabot 1940, nos. 191, 192, 353, 793). Gsell suggested that the name Mazucan is derived from Mazic. This occurs on a Neo-Punic inscription of the Wadi el-Amud, south of the Wadi Sofeggin (Brogan 1964, 49).

We now come to the tribes showing rather closer links with Tripolitania and its eastward extension along the coast of the Greater Syrtis. The Greek nickname of Lotophagi 'lotus-eaters', for the people along the Lesser Syrtis near the island of Meninx (Djerba), does not help. Herodotus also gives the names of Machlyes and Gindanes in this area, but most of these people will have been absorbed into the Libyphoenician group already considered. One name, that of the *Cinythii*, has been found on a boundary inscription near *Gigthis*.

### *Macae*

The most important of the coastal tribes within Tripolitania mentioned by Herodotus is that of the Macae, who were situated on the Syrtic coast west of the Nasamones (Herodotus, IV, 175; V, 42). The Cinyps (Wadi Caam) ran through their land and

<sup>5</sup> Cf. CIL V, 5267, which mentions a prefect of six Gaetulian tribes in Numidia: *nation(um) Gaetulicar(um) sex quae sunt in Numidia*.

<sup>6</sup> Or *Vagi Getuli*(?) = 'wandering Getuli'; see Miller (1916), 949.



he states that they combined with the 'Carthaginians', evidently the neighbouring Lepcitanians, to drive out the Greek Doreius who was attempting to found a colony at the mouth of the river. This large tribe survived far into the Roman period around the western and southern part of the Greater Syrtis (Desanges 1962, 106) and must have been in touch with the inhabitants of the lower Sofeggin and lower Zemzem basins. They may also have been related to the Cyrenaican Macetae 'half-barbarous people' who according to Synesius (fifth century) passed on information to the barbarians beyond the border (Fitzgerald 1926, no. 130).

### *Nasamones, Psylli and Seli*

The Nasamones had a formidable reputation (Gsell 1913-1929, V, 82-83, 85; Windberg in Pauly-Wissowa, XVI, 2, 1935, s.v. 'Nasamones'). Their main territory bordered the eastern part of the Greater Syrtis, and they do not directly affect our problem, though they roamed far to the south and west towards the borders of the Garamantes. They are mentioned in the sixth-century poem of Corippus, the *Iohannid*, still in their old haunts, unless this is deliberate anachronism (Corippus, *Ioh.*, e.g. VI, 198, 552 = Diggle and Goodyear 1970, 120, 135). The Psylli (Herodotus, IV, 173; Hecataeus: *Fragm. Hist. Graecae*, I, 23, no. 303; Pliny, *Hist. Nat.*, VII, 14), from whom an early name of the Syrtic Gulf, Psyllikos Kolpos, was derived, were neighbours of the Nasamones and were said to have been destroyed by the time of Herodotus so that the Nasamones (responsible for their annihilation according to Pliny) had taken possession of their land. They did not, however, completely disappear, and were probably in some sort of subject relationship to the Nasamones.

In the third century A.D. there was a tribe of some importance, the Seli, along the south of the Greater Syrtis, with towns at Macomades Selorum (Zaafran, Sirte) and Digdica or Vigdida Municipium Selorum (perhaps a site on the Wadi el-Hariga) (Peutinger Table: Miller 1916; *Antonine Itinerary*; *Ioh.*, II, 120; Cerrata 1933, 220-224 on Wadi Hariga site; Goodchild 1954a). They were still in evidence in the sixth century, when Corippus says that Digdiga sent a contingent to fight with the Moors against John Troglita. The view that the Seli are the same as the Psylli seems a very reasonable one.

Two minor tribes, the Muduciuiii and the Zamuci, are named on another boundary inscription of A.D. 86-87 found near Zaafran (Sirte) (Reynolds and Ward Perkins 1952, 854).

### *Phazanii, Gamphazantes, Gadabitani and others*

Early in the third century a legionary detachment was stationed at Cydamis or Cydame (Ghadames) (Reynolds and Ward Perkins 1952, 907-909; Goodchild 1954b) which, like the forts at El-Gheria el-Gharbia and Bu Ngem, kept watch where an important route from the south approached Roman territory. Cydamis was, according

to Pliny, a city of Phazania, a land 'in the direction of the African desert beyond the Lesser Syrtis' (Pliny, *Hist. Nat.*, V, 35; Windberg in Pauly-Wissowa, XIX, 2, 1938, s.v. 'Phazania'; Daniels, 1970b, 14 and fig. 1).<sup>7</sup> Phazania then must be the territory between the Gebel Nefusa on the north, the Hamada el-Hamra on the south, and the sand sea on the west, a forlorn desert area with the small oasis of Derg about 100 km. east of Cydamus and the bleak village of Sinaunen half way to Nalut—country not inviting even for nomadic herdsmen. It is usually accepted that the inhabitants of Phazania were the Gamphazantes, a tribe mentioned by Mela (I, 8) and Pliny (V, 8). Procopius places a tribe called the Gadabitani near Lepcis Magna (*De Aedif.*, VI, 4) with a town called Gadabis which Desanges (1962, 16, 22, 91-92, 138 n. 6), thinks could be a version of Cydamis which would move the Gadabitani into old Phazania. Another somewhat elusive Gaetulian tribe called Nathabres is reported between Roman territory and the Garamantes (Tissot 1884, II, 718; Orosius, I, 2, 90). The name Phazania disappears from later Roman sources but by the time of the early Arab historians it reappears as Fezzan, the name they use for the old Garamantic territory.

We hear of various other tribes in Tripolitania during the Moorish wars of the sixth century, among them Ifuraces, Muctuniani, Astrices, etc., but there is no indication how long they had been there.

#### *Arzuges* (Desanges 1962, 78; Romanelli 1926)

A certain unity is given to the Tripolitanian scene by the existence of the Regio Arzugum, despite the problems it raises. By the late fourth-century a distinction was sometimes drawn between Tripolitania proper and the inland zone which was known as the Regio Arzugum (Goodchild 1950a, 30-31; Courtois 1955, 94). The Arzuges or Arzugitani had their own bishops and by the end of the century communication between the Arzugitan and the Tripolitanian (i.e. coastal) bishops was becoming difficult because of intervening barbarian tribes. The two areas, Goodchild suggested, met near Lepcis Magna, where the Gebel reaches the sea, but elsewhere were separated by the plain of the Gefara, and he therefore assumed that at the time in question the Gefara was the region into which barbarian tribes were infiltrating. The Arzugitan bishops could however communicate with Byzacena and Carthage along the old *limes* road of the Gebel. Their *regio* extended to the neighbourhood of the Shott el-Jerid, for we hear of a bishop of Tusuros (Tozeur) travelling to Carthage in 411 and passing through the land of the Arzuges (presumably to take ship from Tacape). A boundary stone found between the *limes* road stations Bezereos and Tibubuci bears the tribal name Arzosei which might perhaps be a variant of Arzuges (Cagnat,

<sup>7</sup> Pliny says that Phazania was separated from the Garamantes by the Black Mountains. The Hamada el-Hamra (Red Desert) is, however, a greyish purple and the real black mountains (the Gebel es-Soda) approach it on the east and south-east.

Merlin and Chatelain 1923, 30). At the other end of the road, at Lepcis, a third-century tomb bears the ethnic Zurgem, which Romanelli thought might be basically the same as Arzugum (Reynolds and Ward Perkins 1952, 729; Romanelli 1925, 165 ff.). The churches of the Sofeggin basin very likely came under the jurisdiction of the Arzugitan bishops, and since St. Augustine, in one of his letters mentions barbarians being admitted into the country of the Arzuges along the frontier to carry out tasks such as crop-watching, it looks as though the *regio* extended right across our pre-desert area (*Epist.*, 93 and 46-7). Corippus, referring to a region whence came some of John Troglita's foes, says that Arzugis "is the name which the ancients gave it" and describes it as *horrida tellus* (*Ioh.*, II, 148). This, as Goodchild said, "is an apt description of the bleak wastes of the Sofeggin and Zemzem basins". If the Arzuges ranged over so great an area they must have been a large confederation of tribes, and the few names of sub-tribes we have, such as M.s.li and T.gl.bi (or N.gl.bi) in the Wadi el-Amud (Sofeggin, first century) (Levi Della Vida 1964a, 58-60) or Motebi (El-Amrouni near Fom Tatahouine, Tunisia, third century) could belong to them (Neo-Punic inscription; see Berger 1895, 73-75).

Not all the tribes of inner Tripolitania need have been as sedentary as the people of the fortified farms and the more productive wadis. Despite the widespread settlement of the Roman period there are still many valleys with no trace of buildings. The semi-nomads could have roamed in these wadis without harming anybody, though clinging tenaciously to their rights in wells, cisterns and sowing-grounds. Outside the frontier where crops could not be raised, there were plenty of big wadis with a certain amount of pasture, especially along the Wadi Bei el-Kebir and among the hills to the south of it (Brogan 1965). Many *cabilas* still inhabit the area, congregating round a few major wells like that of Schweref in late summer when lesser wells and cisterns dry up.

#### *Garamantes* (Daniels 1970a, 1970b; Pace, Sergi and Caputo 1951)

The Garamantes occupied the most habitable region of the Sahara, the Wadis el-Agial and Sciati and the line of oases from the Murzuk neighbourhood to Zuila. They must have controlled the valleys leading northward from Brak in the Wadi Sciati through the western outliers of the Gebel Soda towards the Wadi Bei el-Kebir. The help given to the city of Oea (Tripoli) in A.D. 69 implies that they could reach the Tripolitanian coast at some speed (Tacitus, *Hist.*, IV, 50). It is perhaps worth noting the close connection which is maintained to-day by the inhabitants of southern Tripolitania and Fezzan, by links much older than the motor vehicle (cf. Barth 1857, I, 105-106; Despois, 1935, 121).<sup>8</sup> Zintani and other tribesmen of the Gebel often

<sup>8</sup> In 1958 I gave a lift to a schoolboy returning to El-Gheria el-Gharbia from Mizda who said that he would not find his father at home because he was in Brak, where the family owned palm trees.

own palm trees in Fezzan, and it is still not uncommon for sheep and goats to be driven down to Fezzan from the Gheriat.

Secure in their desert the Garamantes survived for a thousand years in their ancient home. There is plenty of evidence of trade between them and the Roman and Byzantine worlds (Daniels 1970a, 1970b; Pace, Sergi and Caputo 1951; Ayoub et al., 1967). They are mentioned by Corippus and there was a tradition (sixth century) that they had been converted to Christianity by the Byzantines. Coming to the Arab period we are told how Oqba-ibn-Nafi came to Fezzan and how he subdued the King of Germa (De Slane 1852, 309, from Ibn 'Abd al-Hakam). There is no evidence that the king was other than a Garamantian.

#### IV. RAIDERS OF THE LATE EMPIRE

The final tribes to be considered are those nomadic peoples who raided Tripolitania in the last days of the empire and in the end overran it.

*Austuriani* (Ammianus Marcellinus, XXVII, 9,1; Fitzgerald 1926)

In the broad wastes of Syrtica, between Tripolitania and Cyrenaica, where we have already met Macae, Nasamones, Psylli, Seli, Gaetuli and Mazices, we find the Austuriani in the fourth century, and, after them, the Leuathae, Laguanten or Lawata. Who the Austuriani were we do not know. They may simply be a vigorous sub-tribe of some older group now emerging as the leaders, just as the Leuathae in their turn may well be related to the Austuriani. Ammianus tells us that the Austuriani, a barbarian tribe from the borders of Tripolitania "which lived by murder and rapine and was remarkable for the rapidity of its movements, had resumed its habits of pillage and murder after a period of inaction". The occasion of this raid (365-366) was the brutal execution, by burning alive, of one of their chiefs, Stachao, who "by reason of the peace moved freely about in our territory". He had been accused of various crimes and, in particular, of having plotted to bring the Austuriani into the province. The raiders made for Lepcis and pillaged the country round about, capturing some of the leading citizens who happened to be on their country estates at the time. They came again and again, carrying their depredations into the territory of Oea.

In view of their later attacks on Cyrenaica, described by Synesius, it seems obvious that the Austuriani came from the borders of Syrtica. Corippus names them among the Syrtic peoples (*Ioh.*, II, 345). They had a deep, pre-desertic *hinterland* behind them, stretching to the oases of the Giofra (Waddan, Socna, Hun).

The coast along much of the Greater Syrtis is still very important to pastoral tribes. Water is not very deep down and there are thus plenty of wells. Much of the land is gently-rolling steppe and despite its forbidding appearance constitutes a famous

grazing-ground. The Nasamones in the time of Herodotus used to go to Augila (Herodotus, IV, 172) to gather the date harvest, leaving their cattle to graze in the coastlands. The present-day tribe which occupies the Wadi Ghirza near its junction with the Zemzen (the *cabila* Manassala, a sub-tribe of the Orfella) send their camels a hundred miles or more to the Syrtic pastures when the fodder nearer at hand dries up. In the summer hundreds of Bedouin tents may be seen from the road as one drives round the Gulf of Sidra westwards from Zaafran. Here it was, perhaps, that the Austuriani and their kinsmen foregathered, watched uneasily by the farmers of the coast. Then something happened to set the tribes in motion: lean years when even the pastures of Syrtica felt the strain? growing population? or, most likely of all, the relaxation of Roman vigilance.

These questions are not without point, because it is this fourth century which appears to have been the most prosperous era of the Tripolitanian pre-desert. This area may, indeed, have been comparatively little affected because there was obviously more booty to be had around Lepcis and the other rich cities of the coast. It is possible that many of the more remote communities, including some where the Libyan alphabet was used, were not greatly disturbed.

A sudden unexpected raid by mobile forces is very difficult to counter, and by now the desert tribes had acquired large herds of camels and could consequently move rapidly without undue dependence on water points. The camel had been in general use in Cyrenaica and Tripolitania for a couple of hundred years (Demongeot 1962; Brogan 1954). All this time it must have been playing its part in the Saharan trade, and by degrees the desert tribes had managed to build up herds for their own purposes. The demand of the governor, Romanus, in 365, for four thousand camels from the city of Lepcis for use against the Austuriani (Ammianus, XXVIII, 6, 5), may be a measure of his unwillingness to do anything effective against the raiders, but it could also reflect the state of the Roman frontier army which may not have kept abreast of developments in the desert and so did not have an adequate camel-corps for such an emergency as the present one. We may understand more of the conditions which prevailed on the frontier at this time when the later history of the Bu Ngem fort is investigated.<sup>9</sup>

The camel-owning tribes continued their raids westwards and eastwards during the fifth and sixth centuries, profiting by the weakness of Vandal rule and by the disarray of the Roman Empire. The history of inland Tripolitania in the fifth century is obscure. Christianity made further progress and the Vandals had little to do with this remote area. There was plenty of opportunity for more barbarians to enter the country and for some degree of fusion between the newcomers and the tribes which had hitherto been reckoned as within the empire (cf. the views of Oates 1954, 110-113).

For the wars of the sixth century we have the works of Procopius and the curious epic poem *Iohannid* written in the reign of Justin (565-578) by the African, Corippus.

<sup>9</sup> Now being excavated by R. Rebuffat and his colleagues (cf. *Libya Antiqua*, 3-4, 1966-1967).

Corippus pads out his eight books with long lists of dramatic-sounding Libyan personal and tribal names, given in a transliteration of their Libyan form rather than in a Latinized version, and therefore of particular value and interest. Despite its many puzzles and incoherencies the poem is written with genuine enthusiasm for the achievements of its hero, the governor John Troglita, but not without appreciation of the prowess of the Moors, and for Corippus Libya is the most glorious of lands.

*Laguanten (Leuathae)* (Desanges 1962, 102; Bates 1914, 238-239; Courtois 1955, 344-347)

This great tribe appears under a variety of names, and the Leuathae of Procopius are the Laguanten of Corippus. In the single MS. of the *Iohannid* that has survived, the tribe is variously called Laguantes, Laguantan, Laguanten and even Leucada, and it also appears to be the same as the Ilaguas, Ilasguas, or Hilaguas. Furthermore, Corippus several times also groups the Austur (Austuriani) with the Laguanten or Ilasguas, as when he speaks of "the gathering of the invincible Ilasguas, among them the warlike Austur". These Ilasguas, he tells us, had caused trouble in Tripolitania in the time of the Emperor Maximian in the late third century (*Ioh.*, II, 345 [Austur]; I, 480 [Maximian]).

#### *Naffur*

The Naffur were a tribe which fought by the side of the Laguanten and the Frexes. The Frexes were located in Tunisia. The Naffur are thought to be the Nafusa (Nefusa) (Desanges 1962, 122).

It is difficult to assert which tribes had come into the empire from elsewhere (like the Lawata) or which are the old-established inhabitants of Tripolitania. Some of the latter remained in their old homes and made common cause with the wilder barbarians from outside the borders. The men of 'odious Digdiga', probably still the Seli of Vigdida Municipium Selorum (see p. 279) (*Ioh.*, II, 119) had joined their neighbours on the warpath, as had the folk of Tillibaris (*Ioh.*, II, 80) in Tunisia. Corippus makes it quite clear that the people of the Regio Arzugum poured out to aid the Berber hero Carcasan (*Ioh.*, II, 148).

#### *Lawata, Nefusa, Hawara*

With these Berber tribes we come to the end of our story. The Lawata, or Louata, are the people known to Procopius as the Leuathae, to Corippus as Laguanten, and seem at the time of the arrival of the Arabs to be the major tribal group from western and southern Tripolitania, across Syrtica to Cyrenaica and Marmarica (Oates 1954, 110-113; De Slane 1852, 232-233 and 301). The Arab conquerors found some of them leading a settled life in the plain of Barca in Cyrenaica. Here they seem to be the descendants of the raiders of the time of Synesius and of the old



Libyan peasantry. In southern Tripolitania a similar process seems to have been going on; here also many of the Berbers lived in villages and scattered farms—the old farms of the frontier-lands—and here the knowledge of the old Libyan alphabet did not entirely disappear but was passed on to the newcomers (Goodchild 1950b; for Tripolitania cf. Oates 1954).

The genealogies given by Ibn-Khaldun in his *History of the Berbers* relate the Lawata and Nefusa closely as Botr tribes, while they place the Hawara among the Beranes, the other great Berber family (De Slane 1852, 168, 226, 273). This is as artificial as are most similar genealogies. Both Botr and Beranes have a Berr among their legendary ancestors, sometimes claimed to be the same individual, at other times regarded as distinct. Berr, ancestor of the Beranes, is descended from Mazigh, son of Canaan, son of Ham, son of Noah. Sometimes the Botr are given an ancestress Tamzigh, the female form of Mazigh. Ibn Khaldun opts for a common descent of all Berbers from Mazigh (De Slane 1852, 184).

Ibn 'Abd al-Hakam (ninth century) states that at the time of the Arab conquest (643) Nafusa occupied the territory of Sabratha (De Slane 1852, 301; areas of the three tribes, 280-281). The *territorium* of a Roman city could be quite large and it is probable that Sabratha owned not only a section of the Gefara but part of the olive-growing Gebel Nefusa beyond. Another Arab writer Al-Iaqubi, also ninth-century, says that in his day the territory of the Nafusa ran from southern Tripolitania to the neighbourhood of Kairouan (quoted by Desanges 1962, 122). They still occupy the mountains to which they gave their name, though when they arrived cannot be said.

The Hawara are a great name from the time of the Arab conquest, but they are not mentioned in Greek and Roman literature. It seems evident, however, that the Lawata and Hawara must have been closely related. We hear of Lawata around Barca at the time of the conquest and almost immediately afterwards we are told of the vicissitudes of the Hawara of Barca. The Hawara, too, are all over eastern Tripolitania while the Lawata are further west. Ibn Khaldun writes: "At the time of the Muslim conquest, all the tribes bearing the generic name of Hawara, both those who claimed descent from El-Abter and those who claimed descent from Bernes, lived in the province of Tripoli and in the part of Barca which borders it .... Some had permanent abodes, others were nomads" (De Slane 1852, 275).

The traditional genealogies show how the name of Hawar encroaches on other tribes, including on that of Addas, brother of Lawa from whom the Lawata stem. A further passage from Ibn Khaldun may be quoted as another example of the difficulty of reconstructing early Berber history:

The tribes descended from the stock of Hawar are very numerous and the greater number of those who claim descent from Aurigh, father of Hawar, bear also the name of Hawarids because Hawar was the eldest son and because his renown surpassed that of his brothers. Aurigh had four sons, Hawar, Maggher, Calden and Meld. Each of these was the ancestor of several tribes who are called collectively the Children of Hawar. (De Slane 1852, 274).

The Nafusa still live in their Gebel. The Lawata moved further afield, but Lawata tribes are still to be found in the Gebel Demmer in southern Tunisia and up towards Sfax. Eastern Tripolitania is still the home of the Children of Hawar, the Garian and the Msellata, the Tarhuna, the Weshtata, the Mesrata and the Orfella. Some Hawara from Barca (Cyrenaica) pressed down into Fezzan and founded the dynasty of the Beni-Khattab in Zuila; others crossed the Sahara (De Slane 1852, 275, 281). Some Arab writers believed that they gave their name to the Hoggar, because the 'w' and the 'g' can be interchanged (De Slane 1852, 275-276). The Hawara also participated in the general drift westwards which is a marked feature of the movement among the tribes of the dark ages.

#### V. CONCLUDING SUGGESTIONS

We are nearly as far away from an answer to the question of who was responsible for our inscriptions as when we started. The tribal name of the community settled at Ghirza in the fourth and fifth centuries is not known, though from what has been said above (p. 281) the hypothesis that it belonged to the Arzuges may be hazarded. The Christian Arzuges were found along the Tunisian and Tripolitanian Gebel and perhaps in the Sofeggin basin, but the southern fringe of the *regio*, including Ghirza, had remained pagan. At Ghirza Latin is used for the inscriptions on the rich men's tombs put up in the mid-fourth century. Two tombstones of presumably later date were inscribed in Latino-Libyan. In the temple, however, the only inscriptions found are those in the Libyan alphabet. The worshippers who dedicated the little altars probably represent a humbler stratum of society, or at any rate a later generation, than the rich men who built the tombs. Among them were perhaps infiltrators from beyond the frontier, who adopted the alphabet. If one day the altar inscriptions can be read we may learn more about these people.

It will be recalled that the Libyan alphabet was still known and used by the inhabitants of the later house on the site which was occupied in the tenth century. These could well have been Hawara (see above). Whether the inscriptions on two tomb-columns (fig. 24), on doorways and elsewhere are medieval or relatively modern, cannot be said as yet. The alphabet, with some changes and additional letters, is still used by the Twareg in Fezzan as it is at Ghadames and in the Hoggar. Sometimes workmen employed by exploration parties sent down to Fezzan by oil companies have insisted on signing their names in *tifinagh*, and in 1958 there were still graffiti in this alphabet on the walls of the old Italian fort at Brak.



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## DISCUSSION

OLWEN BROGAN: We are out of our depth here with regard to these inscriptions and we hope that some of you are in due course going to help us. Particularly at the little temple, which is dated, I suppose to the fourth and fifth centuries A.D., we have these altars—they are presumably votive altars—and we should so much like to find the name of the God who was being worshipped—it seems reasonable to hope that that might occur. Of the other inscriptions, which may not be quite clear from the paper, there are those altars and the bowl from the temple, which are certainly ancient. And then there are the mediaeval graffiti on the plaster of a house that was built in the ruins of the temple, and in that house we found early Fatimid coins. So presumably the graffiti are mediaeval or very early modern—they were well covered up and I should think probably of the same time as the coins or a little while after. And then the rest of them, nos. 10 onwards (I have given only the best examples out of a total corpus of about 40) occur on buildings: you very often find them on doorways or lintels and they might be of any age. Some of them might be modern Twareg. Miss Reynolds and I had a try to decipher some of them with the aid of Chabot's "*Recueil des inscriptions libyques*", but although he was able to read a great many of the inscriptions from Algeria and Northern Tunisia, there is a horrid little group in Southern Tunisia which beats him. Well now you see, next after Southern Tunisia comes Tripolitania, and we also stuck, only perhaps you would not stick.

BYNON: We are all very grateful for this new source of data and I am personally convinced that, as the size of our corpus grows, we will eventually be able to read at least a large number of these inscriptions.



## SEMANTIC CORRELATES OF BERBER SYNTACTIC PATTERNS

JOSEPH R. APPLEGATE

In historical and comparative linguistic studies emphasis is usually placed on examination of phonological and morphological systems. Syntax and semantics do not usually receive the same degree of attention. Comparison of lexical items, of course, does require some investigation of semantic features, but usually the purpose is to provide a sounder base for statements about similarities of phonological systems. Syntactic comparisons are usually made in general terms to provide the foundation for detailed statements about similarities of morphological systems. It seems, therefore, that the investigation of one area of linguistic structure has been neglected in studies of this type. It is possible that closer examination of syntactic patterns and the semantic correlates of these patterns may provide more important data for historical and comparative studies.

In this paper one phase of this type of syntactic investigation will be presented. It is not possible to present a detailed description of the syntactic structures of all Berber languages at this point, so only samples from a representative group of languages, Shilha, Tamazight, Rif and Kabyle, will be used. The discussion is not intended to lead to a final, definitive statement, but rather point to a direction for further investigations.

First, it is necessary to clarify the description of a basic sentence type. In the descriptions that have been presented by linguists such as André Basset and André Picard, Emile Laoust and Hans Stumme there has been a tendency to assume that the sentences in Berber languages conform to the syntactic rules which can be stated for Indo-European languages. Utterances, aside from those which can be described as exclamations or formulas, are usually described in terms of two constituents or two major phrases corresponding to the subject-predicate construction found in Indo-European languages. This, of course, requires the division of sentences into two phrases; and in terms of the morphological features of the constituents, the nucleus of one will be a member of the noun class while the nucleus of the other will be a member of the verb class. The basic sentence structure is then usually described as NP + VP. Careful examination of utterances in the Berber languages shows that this is not an accurate description, however.

In the Berber languages cited above, a standard feature of the noun morphology is the contrast between nouns occurring as initial constituents of a phrase and those occurring in non-initial positions. This contrast is indicated by the vocalic distinction between /a/ and /u/; the former marking masculine singular nouns in initial position, the latter, those in non-initial position in a phrase. A sample utterance from Shilha *argaz i-ufa ayiul*, 'the man found the donkey', illustrates the fact that a boundary marker between phrases must be placed after the verb *i-ufa*. The form of *ayiul* is the form required for nouns of that category in phrase-initial position. A variant of the sentence illustrates the fact that the first noun *argaz* is a constituent of the phrase which includes *i-ufa*: *i-ufa urgaz ayiul*. The sentence structure may then be described as VP + NP. In the sentence cited above, the NP functions as direct object or complement of the VP. Sentences of this type are found in all of the Berber languages, and the structure may be considered a basic structure for the Berber language group.

There is a second type of structure, however, which must be considered. In this second type of sentence, the verb constituent is missing. In Kabyle, for example, one finds utterances such as *argaz-ag:i d aqbaili*, 'this man is a Kabyle'. Here the constituents are clearly NP (1) and NP (2). The only ambiguous element is the *d* which for purposes of this discussion will be considered a terminal phrase marker. In this case, the two noun phrases are extremely simple in form. In other sentences, the second phrase may be expanded or it may be a slightly different type in which the noun is preceded by a prepositional prefix; *argaz si-bgait*, 'the man is from Bougie'. This type of sentence does not occur generally in Shilha, and one may be led to conclude that the structure in Kabyle is a special type in that language developed by the deletion of the verbal constituent of the initial phrase. Further examination, however, leads to a revision of that conclusion. One of the utterances found in both Kabyle and Shilha shows that the same structure is found in both: *γur-i lflus* (Kabyle) *dar-i lflus* (Shilha), 'I have some money' (lit. 'to me money'). In both cases the two constituents of the utterance are noun phrases, one of which is preceded by a prepositional prefix.

An important distinction between the treatment of such utterances in Kabyle and in Shilha is that utterances of this type in Shilha may be treated as if they were verbal sentences, that is, the negative prefix *ur-* may be added to the noun phrase *dar-i*. In Kabyle, this is not the case. Such utterances may not be negated, nor can they be modified by the addition of any constituent that alters the factual nature of the statement.

It is this second point that is of primary importance for this discussion. The second noun phrase in sentences of the Berber languages is a constituent that is fairly constant, and it may be described as a complement. The basic structure of the sentences may be described, then, as initial phrase + complement. It is the structure of the initial phrase that varies; consisting at times of a noun phrase, at other times of a verb phrase. The major problem in investigations of sentence structure, then is

discovery of factors that determine selection of the verb phrase or the noun phrase as the initial constituent of a sentence. It is possible to state that the selection of a verb as the nucleus of the first phrase indicates focus on an action or process while the selection of a noun as the nucleus of the initial constituent indicates the focus of attention on a person or thing. This statement, while true, is not detailed enough.

The fact that the form of the nominal sentence in Kabyle is restricted to an affirmative form indicates that not only is the focal point of the sentence a person or thing but also that the utterance is used to call attention to something that can be observed by the addressee. In other words, the factual information reported in the sentence can be immediately verified. These two factors seem to be the major determinants of the selection of a noun phrase as the initial constituent of a sentence. If the factual information reported in the sentence cannot be directly observed or confirmed by the addressee, the initial constituent of the sentence will be a verb phrase.

When one considers these facts carefully, the basic distinction between the nominal sentence and the verbal sentence can be described as a distinction between a demonstrative pattern and a non-demonstrative pattern. The demonstrative sentence is the nominal; the non-demonstrative is the verbal. Although the distinction between the two sentence types has been lost in Shilha in the examples cited above (equational sentences contain a verb; *dar-i* may occur with verb affixes), it has been preserved in the language in sentences containing a demonstrative particle *ha-*.

The preceding discussion is intended to illustrate the framework for investigations of semantic correlates of syntactic structures which may provide additional information about relationships between Berber languages. Detailed studies of this type will undoubtedly prove useful in historical and comparative studies, and they should not be neglected.

#### DISCUSSION

HODGE: The nominal sentence, the verbal sentence and the demonstrative variety of the nominal sentence are all very wide-spread in Afroasiatic—at least they occur in Semitic, in Egyptian and in Hausa, and some of the same features are also found. What is the equivalent of the demonstrative sentence here in Kabyle cannot be negated in Egyptian either. You have to use a completely negative construction. In Egyptian you use a demonstrative noun in a very similar manner. In Hausa on the other hand what I think is a comparable situation, namely the *ne/ce* which is a demonstrative, can be negated. So there is no question at all that this is wide-spread in Afroasiatic and I would be very much interested to know the extent to which it is found in other language families.

JEANETTE HARRIES: I would like to say that each of these types of non-verbal sentence which Dr. Applegate discusses is in Tamazight (which is intermediate

between Shilha and Kabyle, at least in location if not exactly in relationship) clearly related to a verbal sentence type and furthermore to a specific verb. Specifically the prepositional predication, corresponding to his *argaz si-bgait*, is related to sentences with the verb *ili* 'to be' (in a temporal sense: to be in a place, a condition) which requires as a complement a prepositional phrase; and the nominal predication, corresponding to *argaz-agi d aqbaili*, is related to sentences with the verb *g* 'to be' (in an inherent sense: to be a Berber, a woman) and this verb takes as a complement a noun or an adjective, that is a predicate-nominative of some sort. Let me explain what I mean by "clearly related": I mean that for every sentence of these two non-verbal types you can elicit from a speaker, or have a speaker accept your production of, a verbal sentence with the verb *ili* if it is a prepositional type of predication, or *g* if it is a nominal type of predication. In fact after formulating rules of grammar for Tamazight for the several kinds of variations of verbal sentences it is possible to add one final rule of grammar which simply says "delete verb", and this will yield exactly the structure of these two non-verbal sentence types. This rule cannot be applied before you have applied other more general rules that give you variations of the basic verbal sentence and I want to show that with the following set of examples: (1) *iga urgaz-a amaziɣ*; (2) *amaziɣ ai iga (> agga) urgaz-a*; (3) *argaz-a amaziɣ agga*; (3a) *argaz-a d-amaziɣ agga*; (4) *argaz-a d-amaziɣ*. I will not try to do it for both types, I will do it simply for the nominal predication. The first sentence then is the verb first order, perhaps the basic sentence order, the one that has the least emphasis on any particular noun in the sentence and seems rather to focus on the verb itself, where *iga* is the perfect form of the verb 'to be'. The second sentence is related to the first; the relationship can be expressed by a rule that says "focus contrastively on the predicate nominative". Now this focus-rule which puts one nominal element of the sentence in initial position and follows it with the relative pronoun, in this case *ai*, is a very general rule which is needed to account for a number of different sentence variations with all kinds of other verbs. In this case you can see from the form *urgaz* in these first two sentences as compared to the last two that it is one of Dr. Applegate's nouns in non-phrase-initial position, as he puts it. It is the subject of the verb following the verb and in that position takes its construct form. A general transformation then accounts for the structure of (2). Sentence (3) is related again to the first two and its relationship can be expressed by a rule of grammar which I call the pre-announced topic rule. It can apply to any post-verbal nominal, which can be pre-posed and takes then its independent form. And this in effect forms the topic of the sentence but it is not the same as the second rule, the focus rule, because there is no contrastive emphasis—no relativization, simply a pre-posing of one nominal element. In this case, for this set of sentences, we have preposed the subject of the verb *g*. Sentence (3a) is insertion of the *d* particle, which one could call an affirmative particle. Now only when these two general rules have been applied and the *d* inserted can you get the final rule applied which says 'delete verb'. That is the last sentence is a non-verbal sentence, it is a nominal predication and it is related to the



other sentences of the set precisely by a rule that says 'delete the verb phrase', which is here the relative pronoun and the verb. And in that way it seems to me one can account for this particular type of non-verbal sentence by an underlying structure which includes a verb and also the elements that trigger these particular transformations, that is that account for the fact that *amaziɣ* is being contrastively focused on and that *argaz-a* the subject is being pre-posed as the topic of the sentence.

BYNON: I wonder whether I could ask you one question, since we are now talking in transformational terms and this Colloquium is on the subject of the history of the language. Now you talk about the deletion of the verb. This is of course a transformational rule—but are you also suggesting that the actual historical situation is reflected in these transformational rules and with their specific order of application?

JEANETTE HARRIES: I would not venture to suggest that. I am only saying that, to account for the structure of these obviously related sentence sets, you can use a series of rules the last one of which will give you the non-verbal sentence and that it would be interesting to examine Kabyle and Shilha to see whether a similar set of rules would apply.

BYNON: If we were to suppose that we have here a reflection of the historical situation, one difficulty of course would be that for the verb 'to be' it is not always the same verb which is used.

JEANETTE HARRIES: It is exactly the same in form as the one which takes a direct object and means 'to do'. Is that what you mean?

BYNON: Yes, and a variety of other forms can be used in this position as the verb 'to be' which might suggest that it is perhaps rather an innovation in the different dialects.

CARNOCHAN: Are all four of these sentences ones which are in actual use, or are (2) and (3) merely stages of your rules to get to sentence (4)?

JEANETTE HARRIES: They are all actual sentences. I will say this however that the first sentence is one that the speaker would prefer not to give. He will accept it but it does not feel normal to him. The speakers whom I consulted on this all apparently want to focus and prepose the subject.

BYNON: These are of course patterns which Professor Galand of Paris has dealt with in a series of papers where he treats them as various degrees of *mise en relief* of the constituents of the clause (*anticipation élémentaire*, *anticipation renforcée*).

APPLEGATE I would just like to comment that—and it is very much the same comment that Dr. Bynon made—although the description presented is very useful in describing a language like Tamazight, I do not think that it does have too much HISTORICAL validity. In other words I do not think there is any historical chain here in the way that you have developed the sentences, and I think possibly the statement that I tried to lead up to in my paper, that basically your sentence in Berber may consist of the two parts ‘topic’, which may be either noun or verb, and ‘comment’, which would be usually a second noun phrase, would be the basic structure and would have some historical validity.

## THE BERBER ELEMENT IN MALTESE

J. AQUILINA

Maltese is an off-shoot of the North African (Maghribi) group of Arabic dialects. Its morphological and syntactic structures remain largely Arabic to this day in spite of the many inroads made upon it by Old Sicilian, Old and Modern Italian and, in our time, also by English. If Maltese can be described as 'structurally Semitic and superstructurally Romance', this is the joint product of the two linguistically more important foreign political dominations in the island. These were the conquest of Malta in 869/870 (forty-three years after the conquest of Sicily) by the Arabs under the powerful Aghlabids who, from their capital al-Qayrawān, dominated during their century of power the mid-Mediterranean, and its conquest (twenty-four years after the Norman conquest of Britain) by King Roger with whom started the Norman domination which was to be followed by that of several other Latin governments—Swabian, Angevin, Aragonese and Castilian (1090-1530). One must assume that, just as the Arab armies which invaded Spain included a large number of Berbers, similarly a considerable number of Berbers must have fought in the armies which conquered Malta.

With the North African Arabs that settled in Malta came a number of originally Latin words, a reminder of Latin Africa, in an Arabic form. Similarly, all those words which one can tentatively trace back to Berber reached Malta in an Arabic form from North Africa, and some perhaps from Moorish Spain. The presence of a number of words of possible Berber origin does not necessarily imply Berber-speaking individuals; the Berber words could easily have come to Malta with the rest of the Maghribi Arabic vocabulary and the smaller group of Arabicized words of Latin origin.

I now give a list of such words, including those given by G.-S. Colin in his article "Mots 'berbères' dans le dialecte arabe de Malte" (1957). Some of the etymological comparisons are merely tentative but it is possible that, from the list as a whole, some further Maltese words will emerge that can be described as directly or indirectly of Berber origin.

- (1) *Arra*! 'gee! get up!' (exclamation to make a horse, etc. run faster). Cf. Berb. *arra*

'hue!' (Beni Iznassen, Rif—Renisio 1932, 332); Renisio compares it with Span. *arre* which has the same meaning, but this interjection is a natural onomatopoeic formation.

(2) *Bahbuḥ*, coll. of *bahbuḥa* 'a cowrie'. Dessoulavy (1938, 3) suggests a derivation 'from its sound when held to the ear', and compares with Ar. *bahḥ* 'to snore', *bahbaḥa* 'to be hoarse', ...; but *bahbuḥa*, Ar. *bahbūḥa*, means 'drizzling rain', connected with the idea of the Maltese verb *bahbaḥ* 'to wash, to rinse, to dabble' (whence *tbaḥbaḥ* 'to take a bath, to splash in the sea'). With Maltese meaning, 'cowrie', cf. Berb. *abāebuc* 'escargot' (Senhaja de Sraïr, Ait Ammart—Renisio 1932, 295, 420).

(3) *Bazuḥlu* (< *bažuḥ* + *lu*) 'one's blue-eyed boy, one near to one's heart'. Cf. Berb. *bazouz* 'ventre, estomac' (Demnat—Boulifa 1909, 343). For similar Maltese composite words, cf. *kačičlu* (< *kačič* + *lu*) 'a man without energy', *ghaxxixlu* (< *ghaxxex* + *lu*) 'a soft, timid man', etc.

(4) *Bebbux* 'snails' is a word which, according to Colin (1957, 9), does not occur in Spanish Arabic. It does, however, occur in Sicilian: *babbalu'ci* or *babbalu'ciu* 'Helix pisana, Muller' "che è comunissima nei dintorni di Palermo" and *babbaluci d'acqua* (Mortillaro 1853, 103-104). This latter corresponds to Malt. *bebbux tal-baḥar*, known amongst the Beni Iznassen as *ajeḡlul lebḥar* (Renisio 1932, 363, 412, 'coquillage, escargot de mer').

(5) *Bexbex*, also *pexpex*, intensive verb 'to urinate frequently'. Cf. Berb. *bešš*, int. aor. *tbešša*, *tbeššaš* 'uriner' (Renisio 1932, 294, 442, 456), also Sicil. *piscia'ri*, Ital. *pisciare* (Mortillaro 1853, 658) the base of which, *piš-*, exists in the Romance and Germanic languages (cf. Engl. *to piss*, Dutch *pissen*, etc.).

(6) *Bilḥaqq*, but also *tabilḥaqq*, the nearest translation of which is 'by the way, now that I come to think of it, I have just remembered (it)'. Cf. Ar.-Berb. phrase *bellḥaqq* 'vraiment' (Laoust 1939, 296).

(7) *Buda* 'Broad-leaved Cat's-tail or Reedmace' (Falzon 1845, 22), *Typha latifolia* L. (Barbera 1939-1940, 245). Cf. Berb. *ābūda*, *tābūda*, current in the Maghrib (Colin 1957, 11). Cf. also Sicil. *bu'da* "sorta d'erba, della quale secca che sia s'intessono le seggiole, e si fanno le vesti ai fiaschi" (Mortillaro 1853, 119). The word is also found as a plant name in Southern Italy, in Calabria and Sicily, where it occurs as *buda* or *vuda*, *guda* in Sardinia, and in the Iberian Peninsula. It occurs also in Low Latin, as we find it in St. Augustine who was of African origin (Epistle 88, l. 6). Maltese *buda* is very likely to be only indirectly Berber. As a Maltese word, it also has the additional meaning of 'a scoundrel'.

(8) *Bumbu*, or *bimbi*, is a child's word both for 'to drink' and 'a drink'. Italian *bubo* is a baby word (onomatopoeic formation) for 'pigeon'. Pitre (1939, 187) gives *mbu* in Sicilian among other words for 'acqua da bere' but this seems to be less close morphologically to the Maltese forms. Cf. Berb. *bubbu* 'breast' (Laoust 1939, 296) and *ammbu* 'bouche' (Siwi—R. Basset 1890, 37).

(9) *Buqexrem* 'Vervain, Pigeon's Herb', of which Falzon (1845, 22) says that it was "erba celebre presso gli antichi, di cui facevano uso nelle loro cerimonie religiose. I medici l'adoprano come vulneraria e febrifuga". Cf. Berb. *bū-qišrem*, similarly described as an efficacious medicinal plant by the 13th century Spanish botanist Ibn al-Bayṭār (Colin 1957, 11).

(10) *Dliel*, coll.n., 'a fine head of hair; head of hair; hair'. Cf. Berb. *adlāl*, pl. *iḍulāl*, 'natte, tresse de cheveux' (Beni Iznassen—Renisio 1932, 309, 456); cf. also *addlal*, pl. *addlalat*, 'tresse de cheveux' (Beni Snous—A. Basset 1929, 53).

(11) *Ehhe* 'yes'. Although this is not recorded in any of the Maltese dictionaries, it is often used instead of *iva* in the sense of 'yes' (Ar. 'aywāh). Hanouz (1968, 225) gives *ihe* 'oui' for Kabyle.

(12) *Farfett* 'a butterfly'. Cf. Berb. *afarṭaṭṭu* 'papillon' (Beni Iznassen, Ait Ouariaghel—Renisio 1932, 299). W. Marçais (1911, 412) gives *fārṭoṭ* " 'agiter convulsivement les ailes ou les pattes (animal égorgé)'; à Tlemcen 'remuer sans cesse (enfant turbulent)'; probablement dénominatif de *frīṭīṭū*, *frī.ṭīṭū* 'papillon', mot d'origine berbère dont des équivalents variés se retrouvent dans tout le Maghreb, à Malte et au Sénégal ...; mais à Tanger, le nom du papillon est *fṛīṭṭo* (avec *r* et *t* non-emphatiques; peut-être par influence de *t*; ...)".

(13) *Fekruna* 'tortoise'. Cf. Berb. *āfkār*, *īfkār* + *-ūn*, Arabic augmentative suffix (Colin 1957, 9); cf. also Libyan Arabic *fākrūna*, *fākrōna* 'tartaruga' (Griffini 1913, 284). As pointed out by Colin, it does not occur in Spanish Arabic. According to Wagner (1932, 652), it occurs in Sicilian as *fukurūna*; it is not however recorded in Sicilian dictionaries. Maltese also has the denominative verb *fekren* (Caruana 1903, 169) 'camminare come la tartaruga, camminare lentamente' also 'to run from one place to another aimlessly; to gad about' with which compare the Arabic denominative verb *fkrn* 'avoir les formes (cheval)' (Beaussier 1958, 759). Dozy (1927, II, 275) gives *fakrūn* and 'afkir 'tortue' and the phrases 'afkir *al-mā*, corresponding to Maltese *fekruna ta' l-ilma* 'turtle', and 'afkir *aṣ-ṣaḥrī*, corresponding to Maltese *fekruna ta' l-art* 'tortoise'. "Ce mot", says Dozy, "est d'origine berbère".

(14) *Fellus* 'a chicken' occurs in the Arabic dialects of the Maghrib and in Spanish Arabic. Cf. Berb. *āfəllūs*, *āfullūs*. Phonemic transition *p > f* is difficult to explain,

but it may be due to Berber influence. Colin (1957, 9) remarks that, although *āfəllūs* may be a Berber loanword from Latin, it is not easy to say whether the word travelled to Malta with the Berbers or with the Arabs. Beaussier (1958, 760) gives *flūs* pl. *flāls* 'poulet, poussin' and Dozy (1927, II, 278) gives *fullūs* as a word existing in the Maghrib in the sense of Latin *pullus, parvus avium*. The word is given also by Schiaparelli (1871, 156) and by Pedro de Alcala (1505); Griffini (1913, 129, 216) gives *fəllūs* for 'pollastrella (gallinaei)'. For Berber, see also Destaing (1920, 227) who gives the forms *tafullust*, pl. *tifullusin*, and Boulifa (1909, 334) who gives *afoulous* (Lat. *pullus*) 'poulet', pl. *ifoulousen* 'petit enfant'.

(15) *Ferkex*, trans. verb 'to scrape the pavement as horses or hens do' (Falzon 1845, 48). Cf. Berb. *aferquš*, pl. *iferqaš* 'pied fourchu d'un animal' (Rensio 1932, 300). Beaussier (1958, 744) gives *frkt* 'disperser ça et là, disséminer, éparpiller, épandre; fouiller, faire des perquisitions' and *frks* 'chercher, rechercher (Tun.)', both words being marked with a cross as barbarisms.

(16) *Fidloqqom* 'Borage'. Cf. Berb.(?) *fūdləqqəm* (Colin 1957, 11). Beaussier (1958, 766) gives *fūdlqqm* 'bourrache' and Dozy (1927, II, 288) *fūdləqqam* 'nom d'une plante'.

(17) *Forn*, pl. *fran*, masc. noun, 'an oven' is of Latin origin (*furnus*, pan-Romance) but may have reached Malta from North Africa with other words like *fellus* 'chicken', *fernaq* (< Lat. *fornax*) 'to flare up (flame)'. Boulifa (1909, 334) gives *afarnou* (root *FRN*) 'four, braise' as well as *aferran* 'four banal, boulangerie'. Beaussier (1958, 745) gives *furn*, pl. *āfrān* 'four à chaud, à briques; fourneau à métaux', and Griffini (1913, 123) *fūrn* 'forno'. But the Maltese word for 'baker' is *furnar* (< Sic. *furnāru*) and not *farrān* as given by Alcala (1505) and by Schiaparelli (1871, 396) who also gives *furnayr* (Span. *hornero*) 'baker' (154).

(18) *Garni*, masc. noun, 'Friar's Cowl'. Cf. Berb. *āyərni*, and the less frequent form *āgərni* (Colin 1957, 11). Hava (1929, 602) gives *qarānyā* 'Cornelian-tree, Dogwood'.

For change of Ar. *q* to *g* cf. Ar. *qarmad* 'to plaster with mud' (Hava 1929, 601) and Malt. *germed* 'to cover with soot', Ar. *qaraf* and *qarraf* 'to peel off the scorch of a wound' and Malt. *giref* 'to scratch'.

(19) *Geddum*, masc. noun, 'a pig's snout; sulky, in the sulks'. Caruana (1903, 188, 191) derives *geddum* from *gidem* 'to bite' as 'l'organo col quale si morde, muso, grugno'. Barbera (1939-1940, 415) proposes Syrian Ar. *qaddūm* but this means 'doloire, herminette, tille de charpentier ou de menuisier' (Barthélemy 1935, 643). Arabic has *qaidūm* for 'a prominent part', but this would give \**qajdum* in Maltese. In the absence of a better explanation, I suggest Berb. *ūdem* 'visage, figure' (Ida ou Semlal—Destaing 1920, 128, 295); *ouddem* 'aspect, forme, visage, face' (Demnat—Boulifa 1909, 365). Destaing (1920, 295) also gives *lguddam* for 'vis-à-vis'. In Sicilian

*guddimu* means 'sulky, frowning', which agrees with Maltese usage. Can we say that the Sicilian word is also of Berber origin and Maltese *geddum* indirectly so through Sicilian?

(20) *Gellux*, also *qellux*, 'a calf'. Cf. Berb. *acällūš* (Destaing 1920, 289; Laoust 1939, 295). Beaussier (1958, 671) gives '*llūš*, pl. '*lālīš* 'agneau (East and Sahara)' and '*llūša*, pl. -*āt* 'agnelle'. He also (671) gives *Alilech* as a personal name and *Gleflex* exists as a nickname in Gozo. Barbera (1939-1940, 416) says 'voce prettamente berbera'.

(21) *Gelmus*. This is the name of a small hill in Gozo. Cf. Berb. *aḡelmus* 'capuchon', the usual meaning (Laoust 1939, 290), but also 'branche' (Senhaja de Sraïr—Renisio 1932, 354).

(22) *Gendus* 'a bull, a young ox'. Cf. Berb. *āḡandūz* 'veau (surtout très jeune)' (Colin 1957, 10); it is also employed by the Aït Ouariaghel, Aït Ammart and Iboq-qoyen (Renisio 1932, 356). Colin points out that we may here have an example of a tendency found in some Arabic dialects to indicate the adult animal by the name of the young animal and he gives examples from the Arabic dialect of Tangier, where the usual Arabic word for 'dog' (*kəlb*) has been replaced by *jro*, literally 'puppy' (Ar. *jarw*, Malt. *ḡeru*), *kəbš* (Ar. *kabš*) 'ram, sheep' by *ḡauli*, literally 'a one year old lamb' and *dik* 'cock' by *faḡrôj*, literally 'young cock'. Both Falzon (1845, 65) and Caruana (1903, 189) describe *gendus* as 'a young ox (bue giovane da razza)'.

Malt. *ḡhoḡol* (< Ar. '*ijl* 'calf', '*ijla* 'heifer') indicates both 'a sucking calf' and 'a young ox or heifer'. Barbera (1939-1940, 417) thinks that Malt. *gendus* is a loan from Berb. *agandūz* etc. and that this is a corruption of Ar. '*allūš*.

(23) *Gorboḡ* 'a hovel'. Dessoulavy (1938, 30) mentions Tunisian *garbāḡi* 'a water carrier' but points out that no connection is apparent and says that the Maltese is very likely related to Ar. *kurbī* 'a hovel' (French *gourbi*); in Tangier *gerbūz* means 'an ugly negro' (Marçais 1911, 439.)

Barbera (1939-1940, 424), suggests Arabic-Persian-Turkish *kurbaḡ* 'a greengrocer's shop' (Hava 1929, 649). One might also tentatively suggest the Berber word, of Arabic origin, *akherbich* 'maison commune de la *takbilt*, *djemâa*' (Boulifa 1909, 336).

(24) *Gremxul*, and *gremxula*, 'a lizard'. Cf. Berb. *āšremšāl*, *āšermšāl*, *āšermšân*, *šeršūmāl* (Colin 1957, 10). The most that one can say about the Berber words suggested by Colin is that they have some phonemes and the basic meaning in common but, as he himself says, the possibility of a relationship between the Maltese and the Berber words remains uncertain. Quite unacceptable, on the other hand, is Barbera's (1939-1940, 424-425) explanation of the Maltese word as a corruption of Ar. *ḡirdawn*

or *ħirdawn*, the name of a large lizard, *Stellio vulgaris* or *Agama stellio*. I suggest the possibility that originally the Maltese *gremxula* was a composite word made up from Ar. *darama* 'courir à petits pas, à pas serrés (se dit, par exemple, du lièvre ou du hérisson)', whence *darrāma* 'lièvre; hérisson' + *šuwul* 'très-agile, très-ingambe' (Kazimirski 1875, I, 692, I, 1291). If this explanation is correct, Ar. *daram* + *šuwul* would easily give *dramxul* which is a dialect variant (Qala, Gozo) of *gremxul*.

(25) *Hafur* 'Oat-grass; oats'. Dozy (1927, I, 386) gives *ħāfūr* in the sense of 'espèce d'origan, *marw*, qu'en Espagne on cultivait dans les maisons ...'; en Égypte 'folle avoine ...'; en Syrie plusieurs espèces de folle avoine portent ce nom'.

(26) *Huxlief* 'hay'. Barbera (1939-1940, 473) derives it from Ar. *ħašš al-a'lāf*, literally 'mietitura di fieni', that is 'mowing, or harvest, of fodder'. Laoust (1939, 291) gives Berb. *aħašlaf*, pl. *i—en* 'herbe'. Maltese also has a verb *ħaxlef* meaning 'to do a piece of work ill and hastily' (e.g. *xoġhol imħaxlef* 'bungled work').

(27) *Inn* or *inni* (spelled *hinn* or *hinni*), adverb current in Gozo, 'there'. Cf. the Berber demonstrative pronouns *enni*, *enn* and *en*.

(28) *Karfa* 'low people, the rabble, the mob; chaff'. Cf. Berb. *ākərfa*, *tākərfa*t (Colin 1957, 15). Beaussier (1958, 859) gives *krfa* 'résidu de balles, débris d'épis et de paille après le vannage'. For the other meaning of *karfa*, 'a person or people of bad character', compare the phrase *karfat al-nās* 'la lie du peuple' (Beaussier 1958, 859), and with Maltese *raġel karfa* 'a man of low character', compare *rjl krfa* 'un homme de rien' (Beaussier 1958, 859).

(29) *Karmus* 'a fig, or other fruit, that does not come to maturity'. Cf. Ar. *karṃōs*, widespread in the Maghribi dialects but unknown in Spanish Arabic (Colin 1957, 12). The collective noun *karṃōs*, whence *karṃōsa* the noun of unity, occurs in the North African dialects, but in the sense of 'normal figs', for which we Maltese use the word *tin* (< Ar. *tīn*). It is interesting to note, however, that while the word is feminine in Tangier, Tlemcen, Nedroma, Mazouna and Algiers, it is masculine in Maltese as it is among the Bedouins and in the rural districts of Algeria. Marçais (1911, 449) considers the origin of the ending *-ūs* (*-ōṣ*) to be obscure. As for the meaning 'unripe fig, not fit for eating', note Beaussier's remark (1958, 862) that *krmūs* derives from Berb. *akrbūz* 'mauvaise figue'.

(30) *Lellux*, coll. noun, 'garden chrysanthemum'. Cf. Berb. *ləllūš*, *āləllūš*, *lullūš*, *tāləllūšt* (Colin 1957, 12). Beaussier (1958, 908) gives *l. llūš* 's. coll., Plante, calendula ..., (Prax)'. *Lellūsa* is employed in Arabic as a personal name, meaning 'a beautiful or glamorous woman', but it is never a personal name in Maltese. The past participle of *lellex* 'to glitter' is *mlellex*, *mlellxa* 'showy', as in *libsa mlellxa* 'a dress with glaring



colours', agreeing with the Moroccan Arabic dialect of the Zaër (Loubignac 1952, 554). Colin sees in *lellux* an affectionate form obtained by adding the suffix -š or -ūš to Maltese *lillu*; note, however, that the vowel *i*, being short, should not bear a circumflex accent—*lillu* 'fine things' (Falzon 1845, 129). Colin compares this word with *lillu* and *ninnu* (hence the false long *ī* in his rendering of the Maltese word) which in the dialects of the Maghrib are applied to 'la lumière vive' and 'feu ardent'. A Berber dialect of Morocco has *ālallu* indicating 'a flower (in general)' (Colin 1957, 12). Bynon, in his paper on Berber nursery language (1968, 113, 146), gives *lullu* as a nursery word with the meaning 'any bright or glittering object (mirror, silver teapot, flower, etc.)'. But the Maltese word *lillu* is neither Arabic nor Berber but Old Italian (15th century) meaning, as in Maltese, 'gingillo, ornamento vano' (Battisti and Alessio 1952, III, 2230) and is to be compared with Medieval Latin *lilium* 'a kind of necklace' (Du Cange 1856, V, 111). Similarly it has nothing to do with Barbera's (1939-1940, 642) colloquial Arabic *al-ḥilū*. Maltese *lillu* occurs mainly in the proverb *Min irid il-lillu, jishar lejlu killu* which Vassalli (1828, 67, no. 600) translates thus: "Chi ama i begli arnesi, che vegli tutta la notte".

(31) *Leqq*, int. verb, 'to shine'. It has no past participle and no derivatives except the adjective *leqqien*. It has no connection with Ar. *laqq* 'to rumble (belly)' (Hava 1929, 692), but cf. '*alaqqa* 'to flash (lightning), to lighten' (Hava 1929, 12), '*alaqa* 'risplendere, ...' (Barbera 1939-1940, 626). Dessoulavy (1938, 59, 57) refers it back to Maltese *lehħ* 'to flash (lightning, blade of a knife, etc.); to be importunate'. Can one think of a possible connection with Ber. *erġ* int. aor. *raqq* 'briller, brûler' (Renisio 1932, 333); Pellat (1955, 133) also gives *rəqq* as the int. aor. of *ərġ* 'brûler'.

(32) *Maqmaq*, int. verb, 'to stutter, talk unintelligibly'. Cf. Berb. *maēmīc* 'bégayer; marmotter, parler confusément' (Renisio 1932, 389). Beaussier (1958, 939) also gives *m'm* 'grogner'.

(33) *Mejxu* (masc.) and *mejxa* (fem.), a name of endearment for 'cat'. Beaussier (1958, 935) gives *mušš* pl. *mšāš* s.m. 'chat (Maroc)'. This is very likely a word of Moroccan Berber origin, the Maltese word sounding like a diminutive form of *mešš* (Laoust 1939, 305). An alternative Maltese word for 'cat', also of endearment, is *pejxu*. Micallef (1959) derives it from Sicilian *muciu* 'cat'. It must be admitted that the pattern of the Maltese word is in a general way non-Semitic.

(34) *Għammem* 'to darken, to make dark or cloudy', whence *sema mgħammem* 'overcast sky'. Hava (1929, 534) gives *ġamma* 'to cover', whence *maġmūm* 'overcast, cloudy (sky); ...'. The Maltese verb is obtained by a reduplication of the same base. Cf. Berb. *ġemmēm* 'être couvert (temps)' (Laoust 1939, 298). Dozy (1927, II, 228) explains the verb with the reduplicated base *ġmġm* 'marmotter' as a variant of *qmqm*, from which derives Maltese *gemgem* 'to grumble'.

(35) *Għattuqa* 'a young hen that has not yet laid eggs' (Falzon 1845, 302). Cf. Berb. *a'atug* 'poussin' (Laoust 1939, 295). Marçais (1911, 377) gives the feminine form *'attūqa* pl. *-āt* 'poule qui n'a jamais pondu'. He says that this word does not occur in Algeria except in the Souf, but is known in Tunisia, Tripolitania and Malta. It is also found in Libya as *'aṭṭūga* but with the meaning 'chioccia' (Griffini 1913, 51), that is 'a brooding hen'.

(36) *Hal Gharghur*, the name of a village in Malta, which has been connected with Ar. *'ārūr* 'disgracing his family' (Hava 1929, 461). It has also been associated with *'ar'ar* 'Juniper tree', but this leaves the long *u* of the final syllable unexplained. One might also tentatively suggest Berb. *aerur*, pl. *icūrār* 'dos' (Beni Iznassen, Rif and Senhaja de Sraïr—Renisio 1932, 379).

(37) *Għazzaz*, trans. verb, 'to grind one's teeth'. Cf. Berb. *əğz*, *ğəzz* 'ronger, grignoter, mordiller (des choses dures)' (Colin 1957, 15); also *εαζεαζ*, int. aor. *τεαζειζ* 'grincer (porte)' (Renisio 1932, 378); cf. also *ğzz* 'ronger, mordiller, manger des choses dures; ... tirer vengeance, satisfaire sa haine' (Beaussier 1958, 706), the reduplication of which *ğzğz* (= Malt. *għazgħaz*) Beaussier (1958, 706) translates 'crier sous la dent (par exemple viande crue, légume vert); crier (souliers neufs, plume en écrivant); grincer les dents'.

(38) *Quccied* 'nits'. Cf. Berb. *gušada*, *gūšad* 'larve de pou' (Znâga of Southern Mauritania) and Algerian Arabic *kuššād* (Colin 1957, 10). The word is also recorded in the Maltese sense by Beaussier (1958, 867), *kššād* 'poux qui viennent d'éclore'. This word has nothing to do with Sic. *cucciddu*, diminutive of *cócciu*, whence Malt. *kocč* 'a handful, many', as suggested by Barbera (1939-1940, 918.)

(39) *Rewrew*, *Tarrewrew*. This is a family nickname in Munxar, Gozo, which has lost its meaning. Cf. Berb. *ráuráu* 'marmotter, parler confusément' (Aït Ouariaghel, Aït Touzine—Renisio 1932, 328).

(40) *Saff*, trans. verb, 'to suck (an orange, sweets, etc.)'. The meaning is peculiar to Maltese but, for a possible association in the meaning, cf. Ar. *sff* 'manger, prendre quelque chose de pulvérulent, comme semoule, farine, etc. à l'état sec' (Beaussier 1958, 475) and Berb. *susef* and *sufes* 'cracher' (Renisio 1932, 313), *asif*, pl. *isaffen* 'rivière, fleuve' (Renisio 1932, 313), *suf* 'gonfler' (Laoust 1939, 309), *sfouf* 'souffler, gonfler' (Boulifa 1909, 369).

(41) *Saqsa* or *staqsa*, trans. verb, 'to ask'. This form, which is very common in North Africa, has developed from Ar. *qaša* and is used largely among the Berbers. Cf. Ar. *sqsā* 's'enquerir, s'informer, prendre des informations; interroger, questionner, demander' (Beaussier 1958, 477); *sek'si* 'interroger, demander, s'informer' (Boulifa 1909, 368); *séqsa* 'demander, interroger' (Destaing 1920, 90).

(42) *Serduq*. This is the only Maltese word for 'cock'. It is current in the Arabic of the Maghreb except in Morocco and at Tlemcen; it is also unknown in the Berber dialects (Colin 1957, 10). This word must have reached Malta from North Africa. The Maltese never use the Arabic word *dik* for 'cock' but they use the word *dika* in the sense of 'hermaphrodite; hermaphroditic'. The Maltese word has a verbal form *isserdaq* 'to overtop, to grow higher than the rest, said likewise of plants; ... to grow proud, overbearing, haughty or saucy' (Falzon 1845, 107), being the 2nd form (< *t* + *serdaq*) of the unused *serdaq*, e.g. *Iżbor id-dielja meta tisserdaq* 'Prune the vine when it spreads out its many branches'. Beaussier (1958, 469) gives *srdūk*, pl. *srādk* 'coq' and the 2nd form verb *tsrdk* (Maltese *isserdaq* for *t* + *serdaq*/k) in the figurative sense of 'monter sur ses ergots'. Far-fetched and unacceptable is Barbera's explanation (1939-1940, 974) of the word *serduq* as the hybridisation of Berber *aiāzīf* 'cock' and Arabic *dik* 'cock'.

(43) *Siġġiewi*. This is one of the place-names in Malta which are difficult to explain. Could it be connected with Berber *asaggi* 'plateau' (Boulifa 1909, 340)?

(44) *Silla* 'sulla, clover'. Cf. Berb. *tāsulla* 'sainfoin' (Colin 1957, 13). As Dozy (1927, I, 670) gives for this plant both *silla* and *salla* '*Hedysarum coronarium* L.' on the authority of Cherbonneau (1849), we must discard Micallef's (1959) tentative derivation of this word directly from Sicilian.

(45) *Ta' Qattagħni* 'a criminal, brigand'. Cf. Berb. *aqēṭṭāe*, pl. *iqēṭṭācen* 'coupeur de routes' (< Ar. *qṭ*); Destaing 1920, 46); *aqṭā* 'brigand, coupeur de route' (Justinard 1914, 120).

(46) *Teftef* 'to feel, to handle or touch lightly; ... to eat with reluctance' (Falzon 1845, 252), whence the Maltese nouns *tentufa* and *teftufa* 'a trifle'. Although still current, neither of these is recorded in the Maltese dictionaries. Cf. Berb. *teftef* int. aor. *teftuf* 'palper' (Renisio 1932, 297). Beaussier (1958, 106) also gives unvowelled *tftf* 'chercher sans y voir, à tâtons', whence *taftūf*, pl. *t.fāt.f* 'petit profit' with which compare Maltese *teftufa*.

(47) *Tengħuda* 'Spurge'. Dessoulavy (1938, 122), finding it difficult to explain its origin, wrote "If the sap of this plant was used as a depilatory, then *naġata* 'to remove hair' might be thought of". Barbera (1939-1940, 1038) suggests Arabic *tākūt*. Dozy (1927, I, 139) gives both *tākūt* and *tākawt* as the Berber name of a plant "proprement 'euphorbe' ". The Maltese, if they want to say that someone is very bad, say *ħażin tengħuda* 'he is a bit of a Tartar'. Cf. also North African *t.lġūda* '...', *Bunium bulbocastanum* L., *Bunium mauritanicum* (Beaussier 1958, 109). Dozy (1927, I, 151) gives *talġūda* 'racine qui ressemble passablement à la pomme de terre, mais dont le gout est peu agréable; les Arabes bédouins s'en nourrissent dans les temps de disette'; Cherbonneau '*Bunium feruloe-folium* Desf.'.

(48) *Tfief*, coll. noun, 'Common Sow-Thistle'. Cf. Ar. *tfâf* (South Tunisia); Berber *tiffâf*, *tiffâf*, *tilfâf* (Colin 1957, 14). Beaussier (1958, 106) gives also unvowelled *t.fâf* 'Laiteron (plante)'.

(49) *Tilliera* 'Viscous Erigeron'. Cf. Berb. *tərrəhla*, *tərrəhlân* (Colin 1957, 14). Barbera (1939-1940, 1067) derives the Maltese word from Berber *terhelân* or *terhelâ* "con cui i Berberi indicano l'Erigeron Aegyptiacus di Linneo o la Conyza-Inula".

(50) *Vavu*, *vava*. This is the Maltese word for 'baby boy, baby girl'. Cf. Sic. *Vava* (= It. *bava*) "voce che usa la plebe in sentimento di ragazzino, e tra loro così si chiamano i bambini, che cominciano a parlare" (Mortillaro 1853, 914). In the dialect of Naples, however, *vava* means 'grandmother' and *vavo* 'grandfather'. For the *v/b* permutation cf. Berber *vava* 'père, papa' (Hanouz 1968, 228) = *bāba* 'mon père' (Chleuh—Destaing 1920, 217).

(51) *Werżieq* 'cricket; grasshopper'. Cf. Kabyle *ārzigān* 'cigale' (Colin 1957, 10). The Maltese word is not recorded in the Arabic dictionaries. Malt. *werżaq*, whence *werżieq*, means 'to scream'. For *werżieq* Dessoulavy (1938, 130) suggests Arabic *'arsaḥ* meaning 'with long skinny legs (but usually of the wolf)'. Barbera (1939-1940, 1118), gives as origin *za'-'aq* from *'az'-'aq*, the 4th form of Arabic *za'-'aq* 'to shriek', but according to Hava (1929, 289) the 4th form means 'to frighten'. I am inclined to explain the Maltese verb *werżaq*, whence *werżieq*, as the result of a fusion of the first syllable of onomatopoeic Arabic verb *walwal* 'to wail' (whence Maltese *werwer* 'to terrify') with *za'-'aq*, reduced to *zāq* by the omission of medial ' as in *rāt* 'thunder' for *ragḥad* (< Ar. *ra'd*) or *bāt* 'he sent' for *bagḥat* (< Ar. *ba'aṭa*).

(52) *Xantkūra* '*Teucrium chamaepitys*, *Ajuga iva*, Ground-pine'. Cf. Ar./Berb. *šəndgūra*, *šəngdūra*, *šəngūra* (Colin 1957, 13); Renisio (1932, 409) gives *šəngura*. As pointed out by Colin, this word does not sound either Arabic or Berber. Caruana (1903, 507) describes *xantqur*, n. unit. *xantqura*, as "campizio, ivertetica, pianta che ama vegetare lungo i ciglioni dei monti, nei precipizi e nei luoghi scoscesi e sterili".

(53) *Xilla* 'cio che, quel che' (Caruana 1903, 517); 'what' (Falzon 1845, 286). This is an obsolete and very strange word which, if it were a combination of *xi* (pronoun which as a rule precedes the noun) and the relative pronoun *li*, would give *\*xili*, or *\*xilli* by emphatic lengthening of the relative pronoun, but never *xilla*. Falzon, in order to illustrate its obsolete usage, gives the saying *Min iḡhid li jrid, jisma' xilla jrid* 'One who speaks without restraint will hear what will displease him' (= Vassalli 1828, 66, no. 583). I suggest Berber *šilla* 'beaucoup' (Laoust 1939, 310). The proverb may originally have read *Min iḡhid li jrid, jisma' xilla ma jrid* 'He who says what he wants will hear many things he would not like to hear' (Caruana 1903, 517).

(54) *Xnakka*, a nickname in Gozo. Cf. Berb. *aṣenkuk*, pl. *iṣenkāk*; *aṣkuk*, pl. *iṣkuken* 'cheveux, chevelure' (Renisio 1932, 350).

(55) *Żelluma* 'a twining or twisting' (Falzon 1845, 292). Cf. Berber *āzellūm*, *tāzellūmt* 'corde-ceinture', *lləm* 'filer', *zalləm* 'retordre de la laine, du poil, etc. pour en faire un fil ou une corde' (Colin 1957, 15). Beaussier (1958, 439) gives *zallūm*, pl. *zlālm* in the Maltese sense of 'corde de laine' and *z.llūma* 'trompe d'éléphant'.

(56) *Żenbil* 'a large basket made of broom carried by beasts of burden for the purpose of loading it' (Falzon 1845, 292-293). Cf. Berb. *azenbil* 'sac fait d'une natte en alfa; vieux bissac' (B. Iznassen and Senhaja de Sraïr—Renisio 1932, 327). Beaussier (1958, 442) gives *z.nbil*, pl. *znāb.l* 'panier en sparte en forme de sac long ouvert sur le côté; il sert à transporter les fardeaux à dos d'âne; on l'y place comme une besace; panier double ... et le précédent (Tun.)'.

(57) *Żoghḥran* 'a species of animalculae generated in stagnant waters' (Falzon 1845, 294). Cf. Berber (Znāga) *zuḡlān* 'moustique marchant sur l'eau vieilles; larves dans l'eau croupie' and remoter forms in Tunisian Arabic (Takrouna) *zoḡlāl* and Kabyle *zuḡlāš*, both meaning 'tadpoles' (Colin 1957, 10-11). Cf. also *zuḡlāš* 'tétard' (Dozy 1927, I, 595) and *zḡlāš*, *zḡlāyš* 'tétards de grenouilles' (Beaussier 1958, 435). Caruana (1903, 532) places *żoghḥran* under the adjective *żghir* 'piccolo' and, under the same radicals, Kazimirski (1875, 1341) gives the corresponding form *ṣuḡrānu* 'petit'. This may be the original adjectival form which became a noun to indicate the minute creatures in stagnant water.

(58) *Żrar* 'small stones'. Cf. Moroccan Berber *āmazṛār* 'gravier, pierraille' (Colin 1957, 16). Boris (1958, 240) gives *zṛār*, pl. *zërra* '...; 2<sup>o</sup> pierre cylindrique et allongée utilisée pour faire du mortier'.

(59) *ajl*, pl. *ajul* 'porcupine', occurring only in Caruana's dictionary (1903, 69). Dessoulavy (1938, 1), puzzled by its origin, writes "No seeming connection with 'ā'il 'a small, mean thing' or ḡāyil 'a stout youth' ". Cf. Berber *ouli* 'sheep' (Hanouz 1968, 225); animal names, like plant names, often change their meaning (cf. *ferdeṭtu*, which in some Berber dialects means 'butterfly' and in others 'swallow').

(60) *Ġanfar* 'to reprimand', for which we suggest, besides Dozy's unvowelled *ṣfr* 'tancer, réprimander, brutaliser, outrager de paroles brutales' (1927, I, 769) proposed by Barbera (1939-1940, 263), also Berber *ačənfir* 'grosse lèvre' (A. Basset 1929, 39).

(61) *Ejja* 'come, hurry up'. Cf. Berb. *īāya*, *ēīya*, *éya* 'su, orsù' (Beguilot 1942, 271); cf. also Ar. *hihi hīhi* 'Get off! Be gone! Again, once more' (Hava 1929, 845).

(62) *Gerżuma, grieżem* 'throat'. Cf. Berb. *agéržum* 'gorge' (Destaing 1920, 143); *gāržûma* 'gola (fauci)' (Griffini 1913, 137); *grjūma*, pl. *grāj̣m* 'gorge, gosier, intérieur de la gorge' (Beaussier 1958, 789).

(63) *Qajjar* 'to dry wet clothes in the sun'. Cf. Berb. *iqqur* 'il est sec' (Renisio 1932, 361), *qqar* 'être sec, sécher' (Pellat 1955, 159).

(64) *Qattus*, fem. *qattusa* 'cat'. Cf. Berb. *qaṭṭūs* 'grosso gatto, gatto selvatico (dal basso latino *cattus*)' (Beguino 1942, 307), given also by Beaussier (1958, 811) as Tunisian, *qṭṭūs*, pl. *qṭāṭīs* 'chat'. Cf. also R. Basset (1890, 40; Siwa, Djerba) *iat'ous*.

(65) *Qorriegħa* 'skull'. Beaussier (1958, 795) gives *qrrū'a* 'sommets, en gén.'. Cf. Berb. *aqarru, aqarruy, aqurru* 'la tête' (A. Basset 1929, 17).

(66) *Żarbun* 'pair of shoes, shoes'. Cf. Berb. *tzarabin* 'pantoufle (jaune); ce mot désigne la chaussure appelée en Orient *markūb* et en Occident *balğa*' (R. Basset 1890, 77). But for a closer word form cf. Ar. *zarbūl, zurbūl* (Dozy 1927, I, 584, who gives detailed etymology).

(67) *Dags* 'size, as much as' and, when used with the pronominal suffixes, 'of the same height, status, age, value or weight as'. This word is not recorded in the Arabic or Berber dialects and a Greek etymology, *táxis*, has been attributed to it (Sutcliffe 1936, 195). The Maltese word occurs in Boris (1958, 61) as *taggez* 'à peu près semblable à ... (par l'âge, la taille, la valeur, le poids)', whence Maltese *daqqas* 'to weigh, to equalise'.

Berber nursery language, of which J. Bynon (1968) has made a careful study, when compared with a similar list of Maltese examples collected by Pullicino (1957), presents many interesting cases of apparent agreement although generally more in sound than in meaning. An onomatopoeic comparison shows a strong subjective element in the more or less arbitrary relationship between the sound and the object to which it refers. Here are some of the examples of these echo-images:

Berber no. 1 *ppappa* 'bread' cf. Maltese *pappa* 'bread'; no. 2 *šišši* 'meat' cf. Malt. *xejxi* 'fine things, light'; no. 14 *diddi* 'wound (cut, prick)' cf. Malt. *mimmi* 'hurt'; no. 20 *zizi* 'goat' cf. Malt. *zizi (tsitsi)* 'hen' and *żizi (zizi)* 'petting'; no. 31 *lullu* 'any bright or glittering object ...' cf. Malt. *lillu* 'fine things (standard language)'; no. 37 *fullu* 'hen' cf. Malt. *fellus* 'chicken (std lang.)'; no. 39 *mimi* 'mouth' cf. Malt. *mimmi* 'pain'; nos. 49, 50 *ppspps, ppssi* 'urine' cf. Malt. *pixxa* 'urine'; no. 56 *sisu, susu* 'couscous' cf. Malt. *zizu* 'meat'; nos. 67, 68 *imiššw* 'cat (standard language)' cf. Malt. *mejxu* 'cat'; no. 72 *mummu* 'baby' cf. Malt. *mamma* 'mother'; nos. 88, 165 *bubbu, boubou* 'breast' cf. Malt. *bumbu* 'drink'; no. 120 *tuttu* 'stick' cf. Malt. *tuttu*

'horse'; no. 124 *kukku* 'sugar' cf. Malt. *kukka* 'egg'; no. 127 *ba'ba* 'clothes' cf. Malt. *bobba* 'dress'; no. 133 *tšūtšu*, *čūču* 'couscous' cf. Malt. *guġu* 'cheese'; nos. 143, 150 *šušu*, *joujou* 'meat' cf. Malt. *guġu* 'cheese'; no. 151 *kaka* 'sugar' cf. Malt. *kaka* 'cake (std. lang.)'; no. 153 *tchi-tchi* 'sit' cf. Malt. *čičči* 'sit'; no. 159 *chichou* 'meat' cf. Malt. *zizu* 'meat'; no. 174 *va'va*, *ba'ba* 'sheep, goat' cf. Malt. *beqqi* 'goat'; no. 185 *khikhi* 'dirt, excrement', no. 189 *q'aq'ah* 'defaecate' and no. 215 *kakkaḥ* 'employed to encourage a child to defaecate' cf. Malt. *kakka* 'dirt'; no. 219 *baa* 'sheep, goat' cf. Malt. *beq* 'sheep'; no. 237 *gadada* 'lamb, kid' cf. Malt. *gidi* 'lamb (std. lang.)', *geduda* 'small lamb'; no. 240 *ennu* 'sleep' cf. Malt. *ninni* 'sleep'.

This comparative lexical survey could be widened by studying also the fairly large number of words which Beaussier (1958) has marked by means of a cross in his Dictionary as being barbarisms. Some of these are comparatively recent loan-words, but a fairly large number of the older ones are common to Maltese and Maghribi. As curious words which have not yet been attributed either a Berber or a genuine Arabic origin, they surely deserve further study in order to establish their origin, migration, diffusion and points of contact both inland and overseas. In the list of words that I have given in this communication, I am quite aware that I have often stopped all too abruptly at the mere *prima facie* phonetic similarity without giving an adequate analysis of the morphology, but I hope I shall do this when I can devote more time and attention to the subject. However, I feel that even the simple list which I have given in this paper points to a considerable inflow of Berber words into Maltese. That is precisely what we would have expected from our knowledge of the history of the Berbers, for they played no mean part in the conquest of the Mediterranean under Arabic, and sometimes as in Spain under Berber, leadership.

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#### DISCUSSION

BYNON: Professor Aquilina's paper adds to the list of Maltese words of supposedly Berber origin. I should perhaps say that, for a Berberist, this sort of evidence could be potentially of quite considerable interest. Those of you who deal with languages which have written historical sources will not perhaps be so conscious of its value, but obviously if we are to accept as exact that the Muslims conquered Malta in the ninth century, that in 1090 the Normans reconquered it, and that from that moment onwards it remained continuously in Christian hands, then any loan-words from Berber which are contained in Maltese Arabic must by necessity have entered Maltese before the close of that period. This would be almost as good evidence in fact as finding an actual document dating from the period and containing the words in question. Now, that would seem to be a valid argument if we lived in the best of all worlds and things were as clearcut as that. I do not know whether any of you may wish to ask about the possibility of large numbers of Muslim slaves having come into Malta in the intervening period or about the detailed ethnic origin of the Maltese population? I am not a historian, but I would like to be assured that there was in fact an absolute cut-off of immigrants from the moment when the Normans conquered Malta.

LESLAU: This is not to answer the historic problem for which I am not a specialist, but it so happens that a few of these words considered by Professor Aquilina as being of Berber origin went as far as Ethiopia. But there they are Arabic loan-words, and therefore I suggest that some of these words are Arabic and not Berber. For instance no. 17 *forn* 'oven', which of course is eventually of Italian or Latin origin. But in Amharic it simply means 'bread baked in an oven' and in this case it comes from Arabic *furnu*. No. 34 *għammem* 'to darken'. Here I can rather use only intuition.

I think it is Arabic and not Berber. It exists in Arabic and my feeling is so to speak here—as one often has to play with intuition when it comes to loan-words—that it is Arabic. No. 56 *ženbil* ‘a large basket’ is Arabic. For that matter it is of ancient origin. I think it is found in Akkadian, is it not? and it also occurs in Amharic. So some of the words may simply be Arabic loan-words without going through Berber.

AQUILINA: I am very grateful for your comments. As a non-Berber scholar I must selfishly confess that, engaged as I shall shortly be with the compilation of a Maltese dictionary, I thought I might pick the brains of the Berberists. That is the reason for this list. If I can refer to specific words, I wish someone would enlighten me about one curious word given only by Caruana, that is no. 59 *ajl* ‘porcupine’. He cannot have invented it, but I have never come across it in Arabic. As to words of Arabic origin, I myself am sure there must be many more than three. Regarding no. 34 *ghammem*, *sema mghammem*, I know that there is the Arabic word *ḡamma* ‘to obscure’ but it is not, as far as I know, recorded as a repeated biradical in Arabic. There again, Arabic dictionaries are never complete. But at least in a Berber dialect it is recorded as a doubled biradical as in Maltese. One word which I would have liked the Berberists to talk to me about is no. 19 *geddum*. I inadequately translated it as ‘a pig’s snout’. Unfortunately, it is also the snout of the human biped, that is to say ourselves! There is also a word in Sicilian, recorded by Mortillaro, *guddimu*, just in the sense that we use it. And I just suspected it might be an eventual development from Berber *udem* ‘face’, and there are also variants in some of the Berber dialects where you get it also like the Maltese with the *g*-. The nearest Arabic word is *qaidūm* ‘the fore-part’, but there again I think in Maltese the diphthong *ai* would have been invariably preserved. Another word where I would welcome the help of the Berberists is no. 31 *leqq* ‘to shine’. In one of the modern Berber dialects Renisio records *raqq* ‘to shine, to burn’. Now by dissimilation you get *r* for *l* very easily in Maltese. Another very doubtful word is no. 33 *mejxu* which is I think the Berber name for ‘cat’.

BYNON: It is not only the Berber one, unfortunately. In fact, it would appear to have come as far as English in the form *puss*. The cat is the latest of all the domestic animals to enter Europe, arriving in fact only during the course of the Middle Ages. English *puss*, Maghrebi Arabic *mušš*, Berber *imiššw*, etc.—[Aquilina: *peyxu* in Maltese, with a *p*-]—have all been traced to, or at least claimed as coming from, the name of the Egyptian cat-goddess *Bubastis* (or *Bastet* or *Pasht*). This seems reasonable enough, since the zoologists tell us that the domestic cat of Europe did in fact apparently originate in North Africa, and that it was probably the ancient Egyptians who domesticated it. In any case words of this form are extremely widespread in the West wherever the cat is found. Whether they are ultimately of Ancient Libyan origin or not is another matter.

AQUILINA: But even so, apart from that, I myself would think it is not Berber really because of its form which appears to be a diminutive form in Maltese.

PARSONS: *mussa* 'cat' appears to be a loan-word in Hausa (Sokoto).

VYCICHL: Just one word on two of the animal names. Late Latin *musco* is 'the mouse-eater'; the *-o* is a formative element as in *Cicero*, *Naso*, *falco* (meaning 'which has nails', from *falx*). No. 4 *bebbux* is a Berber word of Latin origin, meaning 'la baveuse, qui bave'.



## BEGADKEFAT IM BERBERISCHEN

WERNER VYCICHL

Bei einem Studienaufenthalt auf der Insel Djerba im Sommer 1969 hatte ich Gelegenheit, den dortigen Berberdialekt zu studieren. Es handelte sich dabei vor allem um die Mundart des Töpferdorfes Guellala (berb. *Iqellalen*, pl.) im Süden der Insel und zwar um das Quartier der Ifammen (arab. *El-Fahmiyin*). Ein bemerkenswerter Zug des Berberischen von Djerba ist die doppelte Aussprache der sogenannten *Begadkefat*-Laute, die hier (mit Ausnahme rezenter Lehnwörter) erhalten ist.

Unter dem Merkwort *Begadkefat* versteht man in der hebräischen Grammatik die Laute *b, g, d, k, p, t*, die nach Vokalen als Frikative (*ḥ, ḡ, ḫ, ṭ, ṣ*) gesprochen werden. Es heißt also *bētō* 'sein Haus', aber *bě-ḥētō* (fast *bě-vētō*) 'in seinem Haus'. In der Geminat liegt die okklusive Aussprache vor. Neben dem Hebräischen kennt auch das Aramäische die doppelte Aussprache dieser Laute.

Wie es scheint, haben einmal alle Berbersprachen die doppelte Aussprache der *Begadkefat*-Laute gekannt. Heute existiert sie aber nur noch auf Djerba, dann in der winzigen Enklave der Ghomara inmitten arabischen Gebietes im Norden Marokkos (Colin 1929, 43-58) und wiederum in fremdsprachiger Umgebung, bei den Zenaga am Senegal (Nicolas 1953, 175).

In den übrigen Berbersprachen hat sich im allgemeinen entweder die okklusive oder die frikative Aussprache durchgesetzt. So heißt die 'Frau' bei den Kabylen *tamēṭṭut* (frikativ), bei den Beni Mzab *tamēṭṭut* (okklusiv), wogegen es in Djerba *tamēṭṭut* (Anlaut okklusiv, Auslaut frikativ) gesprochen wird.

Als Beispiele dieser doppelten Aussprache seien die folgenden Formen genannt :

Djerba *baḥa* 'mein Vater', kabylich *ḥaḥa*, schilhisches *baba*;

Djerba *yezrid* 'er hat mich gesehen';

Djerba *azirar* 'lang' für ein ursprüngliches *\*azēgrar* (über *\*azēyrar*);

Djerba *afrux-iḵ* 'dein Junge', cf. schilhisches *afrux-ēnnēk*;

Djerba *ēljēnnēt* 'Paradies', aber *ēljēt-uh* 'dieses Paradies';

Djerba *ēzzibḍēt* 'Butter', aber *ēzzibḍēt-is* 'seine Butter' (für *\*ēzzibḍēt-is*).

In Fremdwörtern wird die okklusive Aussprache auch nach Vokalen geduldet.

Wir hatten bereits das Beispiel *əzzibdət*. Ebenso verhalten sich *əlqahwət* 'Kaffee' und *əššəkwət* 'Buttersack' neben *əlqahhəwt-is* 'sein Kaffee' und *əššəkkəwt-is* 'sein Buttersack'. In manchen Fällen stehen zwei Formen gleichberechtigt nebeneinander: *aḥəwwat* und *aḥəwwat* 'Fischer'.

Es gibt zahlreiche Indizien, die vermuten lassen, daß die doppelte Aussprache der *Begadkefat*-Laute einst in der ganzen Berberei verbreitet war. So geht tuareg *ikfē* 'er hat es gegeben' auf *\*ikfa-t* zurück. Die Zwischenstufen *\*ikfa-t*, *\*ikfa-d*, *\*ikfa-y* vermittelten zwischen der ursprünglichen und der heutigen Form.

Manche Formen sind schwer zu durchschauen. So heißt es *ayəddi* 'Hund', aber *taydət* 'Hündin' und *taydət-is* 'seine Hündin'. Hier lag im Femininum ursprünglich *\*taydət* (mit Geminatio) vor. Historisch scheint die doppelte Aussprache der *Begadkefat*-Laute im Berberischen nichts mit der gleichartigen Erscheinung des Hebräischen oder Aramäischen zu tun zu haben. Es handelt sich vielmehr um eine lautphysiologische Erscheinung, wie sie uns auch aus anderen Sprachgebieten, wie dem Keltischen und dem Bantu, bekannt ist. Im Grunde genommen liegt eine Assimilation vor, indem nach Vokalen der Verschluß des einfachen (d.h. nicht-geminierten) *Begadkefat*-Lautes gelockert wurde.

Im Gegensatz zum Hebräischen und Aramäischen nehmen im Berberischen die emphatischen Laute *q* und *t* an der doppelten Aussprache teil. So geht schilhisches *aḡanim* 'Rohr' und *taḡausa* 'Sache' auf *\*a-qanim* (pun. *qanim*, pl.) und *\*ta-qausa* (lat. *causa*) zurück, was bereits Stumme (1899) gesehen hat. Die Endung *-ḡ* der ersten Person Singular wie in *əxdəmməḡ* 'ich habe gearbeitet' (Djerba) geht zunächst auf *\*-qu* zurück und entspricht semitischem *\*-ku*. Altes *t* erscheint kabyllisch als *q* und schilhisches als *q*, wogegen es in den östlichen Dialekten als *t* erhalten ist: siwa *it* 'Nacht', Djerba *it*, schilhisches *it*, tuareg *ehəḡ*, aus *\*a-baṭi*, cf. logone *va'de* (Lukas 1937).

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#### DISCUSSION

BYNON: I wonder whether the fact that certain phonemes have two allophones, one occlusive and one fricative, is really an ancient feature preserved in certain

modern dialects of Berber? It seems to me that this is a feature which can arise very commonly in languages.

VYCICHL: I do not think that it is very old. It must have originated separately in North Africa and in the Near East, and we have similar cases in African languages and in Celtic.





**V**

**CUSHITIC SECTION**



MATERIAL FOR A COMPARATIVE DICTIONARY  
OF CUSHITIC LANGUAGES:  
SOMALI-GALLA COMPARISONS \*

ANDRZEJ ZABORSKI

The first step in recognizing the common descent of two or more languages is usually the recognition of the phonetic and semantic similarity of some of their morphemes. In the case of Cushitic this step has already been made, and discovering sets of regular correspondences between the phonemes of these languages should be the next step.<sup>1</sup> In his article on the position of Galla in the Cushitic group, B. W. Andrzejewski has given his estimation of the resemblance of the Galla and Somali vocabularies as 20 per cent. He stresses, however, the fact that no regular correspondences have been discovered and concludes that "it is possible that simply not enough work has been done yet, but it is equally possible that the relationship between Galla and the neighbouring languages is not that of direct descent from a common parent language" (1964, 137).

How much work has so far been done? As far as the published results show, a great deal is due to the great pioneer of Cushitic studies, Leo Reinisch, who amassed most of the existing lexical materials and made extensive comparisons. It is true that in a number of cases the latter do not conform to contemporary standards of criticism and of linguistic method since he relied, especially in comparisons with other languages of the Hamito-Semitic family, on rather vague resemblances. This made many scholars look with suspicion on Reinisch's work in general in spite of the fact that the majority of his comparisons within the limits of the Cushitic branch are quite acceptable. Most of them are, however, dispersed throughout his books and have not been put together and verified. Next, there is Marcel Cohen's *Essai* (1947) in which Cushitic materials are used only within the broader range of Hamito-Semitic. Dolgopolsky's remarkable article "Materials for the historical comparative phonetics

\* I wish to express my gratitude to Dr. B. W. Andrzejewski who advised me to compile this material.

<sup>1</sup> On the other hand J. Greenberg says that the minimal evidence of genetic relationship need not necessarily depend on the discovery of regular sound correspondences as a certain amount of resemblance involving both sound and meaning discovered in the process of mass comparison cannot be explained otherwise than by genetic relationship (1957, 35-45); cf. Hoenigswald (1960), 135. See also Haas (1966), 116-121.

of the Cushitic languages—labial and dental stops in initial position” (1966) seems to be the only study concentrating mainly on the Cushitic languages and based on material from almost all the languages and dialects which are known. The results attained by Dolgopolsky in discovering regular sets of correspondences are quite convincing and I see no reason to doubt the possibility of establishing such correspondences nor to believe in the necessity of having recourse only to typological comparisons which may be merely supplementary.<sup>2</sup>

The purpose of this communication is to show that the basic vocabularies of Galla and Somali contain a number of common items and that it is possible to discover regular correspondences between the phonemes of these languages (not ‘exceptionless’ sound laws in the neogrammarian sense).<sup>3</sup> The results of Reinisch, Cohen and Dolgopolsky have been taken into account, and for comparing the basic vocabularies the list of 200 words compiled by Swadesh has been used as a handy reference. Finally the dictionaries by da Thiene (1939), Foot (1913), Viterbo (1892) and Tutschek (1845) as well as M. M. Moreno’s grammar of Galla (1939) have been checked in the search for other cognates with Somali, for which Reinisch’s (1902) and Abraham’s (1962) dictionaries have been consulted. The fragmentary nature of this lexical material and the phonetic inaccuracy of most of it puts obvious limitations on the results attained, but this cannot be considered an argument against such comparisons in general. A linguist working on comparative Cushitic is in a position somewhat similar to that of a specialist working on extinct ancient languages known from a limited corpus written in a foreign script not well adapted to the particular phonemic systems of these languages. It will be a long time before new, reliable and exhaustive data on most of the Cushitic languages are collected, and there is a serious danger that some of these languages will die out before modern linguists describe them, replacing the imperfect notes made by nineteenth century linguists and amateurs.

Most of the correspondences seem to be quite convincing, and the existing variations and deviations may be due to the limitations of the available sources (phonetic inaccuracy, dialect differences and large lacunae in the data), and to the fact that since the examples gathered are too small in number it is impossible to make any statement about the conditioning factor in some cases. Only consonant correspondences have been taken into consideration.

The respective sets of cognates are as follows:

- (1) Galla /b/ corresponds to Somali /b/:

<i>barriti</i>	‘morning’	<i>berrito</i>
<i>baala</i>	‘feather’	<i>baal</i>
<i>baru</i>	(‘to learn’)	<i>bar</i> (‘to teach’)
<i>buub</i>	‘nervous, wild camel or horse’	<i>buub</i>

<sup>2</sup> See Jakobson (1958), 17-25.

<sup>3</sup> Cf. Coseriu (1958), §§ 4.4.1.-4.4.8.

<i>balla</i>	'wide'	<i>balla-ḍan</i>
<i>roba</i>	'rain'	<i>roob</i> (Cohen 1947, 299)
<i>abba</i>	'father'	<i>abbe</i>
<i>dubo</i>	'tail'	<i>dib</i> (Dolgopolsky 1966, 76)

- (2) There are some examples of Galla /p/ corresponding to Somali /b/:

<i>kope, kobe</i>	'sandal, shoe'	<i>kab</i>
<i>dippo</i>	'narrow'	<i>duuban</i>
<i>sipraar</i>	'skin used as a con- tainer for milk'	<i>sibraar</i>

Moreno (1939, 27) says that /p/ and /ḗ/ alternate with /b/ in Galla. On the other hand Somali /b/, /d/, /ḍ/, /j/ and /g/ are devoiced in final position (Armstrong 1934, 119-124).

- (3) Galla /f/ corresponds to Somali /f/:

<i>fago</i>	'far'	<i>fog</i>
<i>fur</i>	'open, set free'	<i>fur</i>
<i>fula</i>	'face'	<i>fool</i>
<i>faando</i>	'manure, dung'	<i>faanto</i>
<i>afan</i>	'language, mouth'	<i>af</i>
<i>afur</i>	'four'	<i>afar</i>
<i>tufa</i>	'to spit'	<i>tuf</i>

- (4) Galla /m/ corresponds to Somali /m/ in initial position:

<i>malu</i>	'to think, suppose'	<i>malee</i>
<i>moye</i>	'mortar'	<i>mooye</i>

- (5) Galla /m/ corresponds to Somali /n/ in final position since Somali /m/ does not occur finally:

<i>tum</i>	'to strike'	<i>tun</i> (Dolgopolsky 1966, 79)
<i>kumaa</i>	'thousand'	<i>kun</i>

- (6) Galla /m/ corresponds to Somali /b/ in intervocalic(?) position:

<i>lamma</i>	'two'	<i>laba</i>
<i>gurmu</i>	'shoulder'	<i>garab</i>
<i>-tama</i>	'ten'	<i>toban</i> (Dolgopolsky 1966, 78)

- (7) Galla /w/ corresponds to Somali /w/:

<i>warana</i>	'spear'	<i>waran</i>
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<i>warab</i>	('to bring water')	<i>warabi</i> ('to water')
<i>wanne</i>	'heart'	<i>wadne</i> (Galla <i>d</i> + <i>n</i> = <i>nn</i> ; Moreno 1939, 28)

- (8) Galla /d/ corresponds to Somali /d/:

<i>dida</i>	'to refuse'	<i>diid</i>
<i>duulu</i>	'to attack'	<i>duul</i> (Dolgopolsky 1966, 74)
<i>dilli</i>	('battle')	
<i>duula</i>	('army')	<i>duulaan</i> ('raiding-party')
<i>adada</i>	'paternal aunt'	<i>eeddo</i>
<i>dunduma</i>	'anthill'	<i>dundumo</i>
<i>adara</i>	'paternal uncle'	<i>adeer</i> (Cohen 1947, no. 327)
<i>adema</i>	'to go, depart'	<i>aad</i>

- (9) There are some examples of Galla /d/ corresponding to Somali /t/:

<i>daka</i>	'to go'	<i>tag</i>
<i>faando</i>	'manure, dung'	<i>faanto</i>

- (10) Galla /d/ corresponds to Somali /ḍ/:

<i>duma</i>	'to finish, complete'	<i>ḍan</i>
<i>damu</i>	'to announce, inform'	<i>oḍan</i> 'to say'
<i>dippo</i>	'narrow'	<i>ḍuuban</i>
<i>daraa</i>	'clothing'	<i>ḍar</i>

- (11) Galla /t/ corresponds to Somali /t/ in initial position:

<i>tufa</i>	'to spit'	<i>tuf</i> (Cohen 1947, no. 319)
<i>tafki</i>	'flea'	<i>takfi</i> (Dolgopolsky 1966, 78)
<i>tolla</i>	'kind'	<i>tolmon</i> (Dolgopolsky 1966, 79)

- (12) Galla /t/ corresponds to Somali /d/ finally and intervocally(?):

<i>gotu</i>	'to dig'	<i>qod</i>
<i>bita</i>	'left'	<i>bidih</i>
<i>muta</i>	'awl'	<i>mud</i>
<i>sadet</i>	'eight'	<i>sidedd</i>

- (13) Galla /s/ corresponds to Somali /s/:

<i>soofu</i>	'file'	<i>soofe</i>
<i>sagal</i>	'nine'	<i>sagaal</i>
<i>sora</i>	'food'	<i>soor</i>
<i>soba</i>	('to lie')	<i>sab</i> ('to flatter')
<i>sosoba</i>	('to deceive, flatter')	<i>sassab</i> = <i>sab</i>

- (14) Galla /n/ corresponds to Somali /n/:

<i>namaa</i>	'man'	<i>nin</i>
<i>finno</i>	('smallpox')	<i>fin</i> ('pimple') (Dolgopolsky 1966, 61)
<i>manaa</i>	'house'	<i>miin</i> (Dolgopolsky 1966, 66)

- (15) Galla /l/ corresponds to Somali /l/:

<i>lulu</i>	'to shake'	<i>lul</i>
<i>lafe</i>	'bone'	<i>laf</i>
<i>ilma</i>	('son')	<i>ilmo</i> ('child')
<i>olu</i>	('to stay')	<i>ool</i> ('to live, dwell')
<i>olla</i>	('village')	
<i>labbe</i>	'heart'	<i>laab</i> (Cohen 1947, no. 443)
<i>ḍal</i>	'to give birth'	<i>ḍal</i>
<i>tuul</i>	'to heap up'	<i>tuul</i>
<i>sol</i>	'to roast'	<i>sol</i>

- (16) There is one example of Galla /l/ corresponding to Somali /ḍ/:

<i>fila</i>	'to comb'	<i>fiḍ</i> (Dolgopolsky 1966, 59)
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- (17) Galla /r/ corresponds to Somali /r/:

<i>ree</i>	'goat'	<i>ri</i>
<i>arredu</i>	'to run'	<i>orod</i>
<i>rimaa</i>	'pregnant animal'	<i>riman</i>
<i>aria</i>	'to drive away'	<i>eri</i>
<i>arg-</i>	'to see'	<i>arag</i> (Cohen 1947, no. 46)
<i>arbi</i>	'to weave'	<i>arbi</i>
<i>bururi</i>	'fat'	<i>buura</i>
<i>bora</i>	('yellow, chestnut')	<i>bora</i> ('gray') (Dolgopolsky 1966, 57)
<i>kora</i>	'saddle'	<i>koore</i>

- (18) There are also some examples of Galla /r/ corresponding to Somali /d/:

<i>miriga</i>	'right'	<i>midig</i>
<i>torba</i>	'seven'	<i>toddoba</i>

- (19) Galla /ḍ/ corresponds to Somali /ḍ/:

<i>ḍeraa</i>	'long'	<i>ḍeer</i>
<i>ḍiga</i>	'blood'	<i>ḍiig</i>
<i>ḍagan</i>	('to hear')	<i>ḍeg</i> ('ear') (Dolgopolsky 1966, 82)
<i>miḍaan</i>	'grain'	<i>miḍ</i>

<i>ḍala</i>	('wife')	<i>ḍal</i> ('to give birth')
<i>ḍiyo</i>	'near'	<i>ḍow</i>

- (20) There is one possible example of Galla /ḍ/ corresponding to Somali /g/:  
*naḍḍen* 'woman' *naag* (cf. Moreno 1939, 28)
- (21) There are some examples of Galla /ṭ/ corresponding to Somali /ḍ/:  
*haṭiso* 'sneeze' *hiṇḍiso*  
*haṭan* 'to sweep, dust' *ḥaaḍ*
- (22) Galla /ch/ corresponds to Somali /sh/ in one example:  
*gachana* 'shield' *gaashaan*
- (23) There is also one example of Galla /ch/ corresponding to Somali /q/:  
*chenina* 'to bite' *qaniin*
- (24) Galla /ç/ corresponds to Somali /j/ in one example:  
*çhapu, çhabu* 'to break' *jab*
- (25) There is one possible example of Galla /ç/ corresponding to Somali /ḍ/:  
*mičč'in* 'to wash' *mayḍ* (Cohen 1947, no. 487)
- (26) Galla /j/ corresponds to Somali /j/:  
*jira* 'to be, live' *jir*  
*jilba* 'knee' *jilib*  
*jaala* 'friend' *jaal*  
*injerani* 'louse' *injir*  
*jagno* ('brave, courageous') *jigin* ('brave warrior')
- (27) There is one example of Galla /j/ corresponding to Somali /l/:  
*ijja* 'eye' *il* (Cohen 1947, no. 63)
- (28) Galla /sh/ corresponds to Somali /sh/:  
*shan* 'five' *shan*  
*shimbirro, simbirro* 'bird' *shimbir* (Cohen 1947, no. 307)
- (29) There is an example of Galla /s/ corresponding to Somali /sh/:  
*baso* 'cooked grain' *busha* (Dolgopolsky 1966, 51)
- (30) Galla /y/ corresponds to Somali /y/:



<i>biye</i>	'water'	<i>biyo</i> (Cohen 1947, no. 406)
<i>fay</i>	'to be healthy'	<i>fay</i> (Dolgopolsky 1966, 61)

- (31) There is also an example of Galla /y/ corresponding to Somali /r/:

<i>biya</i> = <i>bira</i> (southern 'earth'	<i>ber</i> 'field'
Galla)	<i>berri</i> 'country'

- (32) Galla /k/ corresponds to Somali /k/ in initial position:

<i>kalee</i>	'kidney'	<i>kelli</i>
<i>kenna</i>	('to give')	<i>keen</i> ('to bring')
<i>kufu</i>	'to fall'	<i>kuf</i>
<i>kana</i>	'this'	<i>kan</i>
<i>kumaa</i>	'thousand'	<i>kun</i>
<i>takku</i>	'span'	<i>tako</i>

- (33) Galla /k/ corresponds to Somali /g/ in final position:

<i>ilka</i>	'tooth'	<i>ilig</i> (Cohen 1947, no. 441)
<i>luka</i>	'leg'	<i>luug</i> (Cohen 1947, no. 419)
<i>daka</i>	'to go'	<i>tag</i>
<i>buke</i>	'flank'	<i>bog</i> (Dolgopolsky 1966, 55)

- (34) There are two possible examples of Galla /k/ corresponding to Somali /q/:

<i>kuffau</i>	'to cough'	<i>qufa</i>
<i>bukaw</i>	('to sour')	<i>baq</i> ('to be curdled') (Dolgopolsky 1966, 50)

- (35) Galla /g/ corresponds to Somali /g/:

<i>gubu</i>	'to burn'	<i>gub</i>
<i>galaa</i>	'camel'	<i>gal</i>
<i>galu</i>	'to enter'	<i>gal</i>
<i>gababa</i>	'short'	<i>gaab</i>
<i>eeg</i>	('to watch')	<i>eeg</i> ('to look at, inspect')
<i>gargara</i>	'to help'	<i>gargar</i>
<i>diiga</i>	'blood'	<i>diig</i>

- (36) Galla /q/ corresponds to Somali /q/:

<i>qabu</i>	('to have')	<i>qabo</i> ('to hold')
<i>qori, qorani</i>	'bit of wood'	<i>qori</i>
<i>qoru</i>	'to carve'	<i>qor</i>
<i>baqa</i>	('fear')	<i>baqo</i> ('to fear')
<i>doqe</i>	('mud')	<i>doqo</i> ('mud'), <i>doogo</i> ('turbid water') (Dolgopolsky 1966, 83)

- (37) There is an example of Galla /q/ corresponding to Somali /g/:

<i>maqa</i>	'name'	<i>maga'a</i>
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Cf. also Cohen (1947, no. 198).

- (38) Galla has zero corresponding to Somali /h/ in final position:

<i>baa</i>	'to go out'	<i>bah</i>
<i>ɖaga</i>	'stone'	<i>ɖagah</i>
<i>sadi</i>	'three'	<i>saddeh</i>
<i>bita</i>	'left'	<i>bidih</i>
<i>malla</i>	('abscess, ulcer')	<i>malah</i> ('pus')
<i>naa</i>	'to be afraid'	<i>nah</i>

- (39) Galla has zero corresponding to Somali /'/:

<i>arraba</i>	'tongue'	<i>'arrab</i>
<i>adii</i>	'white'	<i>'ad</i>
<i>anan</i>	'milk'	<i>'aano</i>
<i>ela</i>	'well'	<i>'eel</i>
<i>adu</i>	'sun'	<i>'ad'eed</i> (redupl.)
<i>ɖama</i>	'juice'	<i>ɖa'an</i> (Dolgopolsky 1966, 86)
<i>ɖiɖ</i>	'to tremble'	<i>ɖa'ɖa'</i> (Dolgopolsky 1966, 82)

- (40) Galla /'/ corresponds to Somali /'/:

<i>sa'a</i>	'cow'	<i>saa'</i>
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- (41) Galla has zero corresponding to Somali /h/ in the initial position and between vowels /?/:

<i>aye</i>	'mother'	<i>hooyo</i>
<i>abara</i>	'curse'	<i>habaar</i>
<i>bollo</i>	'hole in the ground'	<i>bohoh</i> (Dolgopolsky 1966, 56)
<i>soda</i>	('father-in-law')	<i>soddoh</i> ('mother-in-law')
		<i>soddog</i> ('father-in-law')

Galla /h/ often alternates with zero in initial and intervocalic positions (see Moreno 1939, 27).

- (42) Galla /h/ corresponds to Somali /h/:

<i>haro</i>	'lake'	<i>haro</i>
<i>hubado</i>	('to understand')	<i>hub</i> ('to be sure') (?)

- (43) Galla /h/ corresponds to Somali /h/:

<i>hoqu</i>	'to scratch'	<i>hoq</i>
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<i>hiḍu</i>	'to tie'	<i>hiḍ</i> (Cohen 1947, no. 51)
<i>hidda</i>	'root, vein'	<i>hidid</i>
<i>hera</i>	'custom, customary law'	<i>heer</i>
<i>hama</i>	'bad'	<i>hun</i>
<i>halelaa</i>	'to wash'	<i>hal</i>
<i>hata</i>	'to steal, rob'	<i>had</i>

It should be very strongly emphasised that this is only a preliminary study and some cognates may have remained undetected. It may also turn out that some comparisons will be seen to be faulty, but still I hope that the rest may serve as a basis for those who will try to discover more remote regularities.

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#### DISCUSSION

JOHNSTONE: A very small point indeed. In set (10) the word *daraa* 'clothing' is almost certainly an Arabic loan-word and presumably Dr. Zaborski would not wish to leave it in his list.

PETRÁČEK The material of this paper is extremely interesting from the point of view of the history and development of the Cushitic system. For example at the end of this very useful list we can see how the laryngals /h/ and /' / of Somali have developed towards ZERO in Galla—which was of course the subject of our discussion yesterday.

LESLAU: Another point of detail. The question is, in the correspondences to what extent one should in principle make a distinction between 'native' or 'indigenous' words and loan-words. For instance in set (17), *kora* 'saddle' in Galla and *koore* in Somali is probably of Arabic origin—it also occurs in Amharic where it is possibly an Arabic loan-word. Well, here the correspondence is 'normal'. Another loan-word, in set (21) *haṭiso* 'to sneeze', is I think Semitic and not Cushitic; cf. Arabic *aṭasa*, Amharic *anāṭṭäsä* and here the correspondence is not 'normal'. So what I am asking is whether somehow in the final analysis one should not make a distinction between how the phonemes are treated in native words and in loan-words.

ANDRZEJEWSKI: Is there not a possibility that some of these words, except of course for the obvious loans from Arabic or Semitic, may be Cushito-Semitic or Hamito-Semitic cognates?

LESLAU: Of course. But then the Cushitist would have to show it. For instance let me turn again to the word for 'to sneeze'. We know it occurs in all the Semitic languages, so that we know that it is not a loan-word in Amharic but a good Semitic word. If it were also found in the other Cushitic languages outside of Somali one would have to ask oneself "is it proto-Semito-Cushitic?". But if it were found only in Somali, to the exclusion of the other Cushitic languages perhaps the obvious solution would be that it was a loan-word from a Semitic Ethiopian language. So Somali alone cannot testify to the native character of the word: one would have to examine the same root in the other Cushitic languages.

PARSONS: I am particularly interested in *tufa* in set (3) because it is pure Hausa too. It is obviously an onomatopoeia and I would never suggest it was a loan-word from Galla to Hausa or Hausa to Galla. Another obvious onomatopoeic form is in set (21) *haṭiso*, which in Hausa is *atishawa*, surprisingly like the English nursery form.

KAYE: This word *tufa* etc. is supposedly listed in all Semitic etymological dictionaries. Ancient Egyptian has just *tf*, Arabic has *taffa*, *yatuffu* with a good tri-consonantal 1-2-2 root, and so forth. It is a very difficult question whether these things are really onomatopoeic or whether they are just good Semito-Hamitic roots.

PALMER: My own feeling on this would be to tip what Professor Leslau said almost upside down—to say that perhaps we ought to be looking for the place in which the correspondences do NOT make quite so much sense or where there are oddnesses of the correspondences, and from there to go on and ask ourselves “why?”. In other words: first of all to get some detailed analysis of what does not really work in a simple correspondence statement, but also to be very careful incidentally to notice that one gets the same KIND of correspondence (*d:t*, *b:p*, etc.) which is not brought out at all well in this kind of list (although of course in Indo-European it is very obvious—I mean things like Grimm’s Law are of this kind). But what I should have thought the descriptive linguist ought to be doing with languages of this kind, taking two languages like Galla and Somali, is setting out a whole list of the correspondences in fairly general terms, i.e. not sound with sound but very often feature with feature, and then asking himself why in many cases it seems to work one way and in other cases seems to work another way. And if we have enough of that kind of material, probably for a number of different languages, then we can work back and say: ah, then this cannot be, or is presumably not, a matter simply of the relationship between two Cushitic languages, but probably goes back to something much earlier, or it may be a matter of loaning, etc. So I would agree with what Professor Leslau has said but perhaps I would in a certain sense want to turn it upside down.

ANDRZEJEWSKI: May I add that in a letter Dr. Zaborski mentioned to me that he naturally thinks of his paper as only the first step in the direction which both Professor Leslau and Professor Palmer have mentioned.



## GENDER IN CUSHITIC

G. R. CASTELLINO

The present paper does not constitute a complete and finished piece of research into the subject it proposes to investigate. The purpose aimed at is simply to supply, where available, such data as are deemed necessary and sufficient in order to assess and clarify the part played in the several sub-groups of the Cushitic languages<sup>1</sup> by gender, consideration being mainly given to its morphological exponents and semantic implications. We shall first give a systematic presentation of the data as described for those sectors of the grammar that are affected by and serve to express gender, namely the noun, the pronoun and the verb. Once the morphological facts and their immediate semantic values have been assembled, we shall endeavour to determine on a wider basis the essential concepts that underlie the structure of gender in the languages we are investigating.

It is hoped that whatever results may be achieved for Cushitic will be of some

<sup>1</sup> For Cushitic we have followed Moreno's classification into four main groups: North, Central, Eastern and Western (Moreno 1940, 319 ff.). The same classification was originally adopted by Greenberg (1955, 51), and by Tucker and Bryan (1956, 118): "with some modifications worked out in collaboration with him [Moreno]". In a more recent work by the same authors (Tucker and Bryan 1966, 2) the larger unit Cushitic (32) was given a new subdivision into three groups: (a) orthodox Cushitic—languages accepted by all as Cushitic; (b) partially Cushitic languages—corresponding largely to Moreno's Western Cushitic; (c) languages with little or no claim to be Cushitic: Konso-Geleba. In a more recent article (1967, 655-680) Tucker discusses whether some of the 'fringe' languages should be included or not within Cushitic. Although the question of these fringe languages is an important one in respect to the classification of Cushitic, it does not directly affect our inquiry as we are not aiming at an exhaustive treatment of the whole Cushitic family. On this same topic see also Bender *et alii* (1969ff.), 31 where percentages of basic vocabulary shared by seventeen Cushitic languages are given; the West Cushitic group of Moreno is not accorded membership within the Cushitic family and a new group is set up, called Omotic. The differences revealed by the comparison of Omotic with Cushitic are born out by the findings of our own inquiry into gender, since whenever we touched upon the area of the Omo languages material for comparison became scanty or divergent. The authors' classification can be seen from the diagrams given on p. 14 for Cushitic and on p. 20 for Omotic although these diagrams are not to be taken as definitive. In our own analysis we have followed the order of the main groups and have labeled them by means of the Roman numerals I to IV. As regards orthography, we have followed that of the sources in order to avoid confusion.

value in the better appraisal of problems of gender in the larger family of Hamitic and Semitic.<sup>2</sup>

A short bibliography is appended in order to show upon what material—unfortunately not always the latest and the best—we have based our study and in order to permit its verification. The usefulness of, if not the necessity for, further study of the feminine in Cushitic was stressed some years ago by Moreno in his description of Sidamo (1940, 23, n. 1).

## I

### *Gender in the noun*

The investigation of gender must obviously start from the noun. In Cushitic, as in many other languages, NATURAL GENDER occupies a prominent place. In the case of

<sup>2</sup> During the Colloquium strong objections were raised against the possibility of carrying out any comparison between Akkadian—and, indeed, Semitic in general—on the one hand and the languages of Northern Africa on the other, both on account of the time-lag involved and of the geographical distance separating the areas concerned. On an earlier occasion, other scholars found fault with the method followed in our work on Akkadian (Castellino 1962). As regards this latter it may be remarked that in some cases the criticisms betray that the reviewers, although quite at home in the field of Semitic proper, have when it comes to the African situation either a rather long distance knowledge or none at all. On the other hand the objections based on chronology and geography came from someone who is highly competent in the languages of North-Eastern Africa. If we had only the modern dialects of Arabic to work with the time-lag between Akkadian and Arabic would be the same as that which exists between Akkadian and the languages of Northern Africa. Should we on this account exclude them from comparative Semitic linguistics? Besides, a comparison based on the structural components is always both possible and legitimate. We say this in order to make clear our position, but we must add that the objections referred to, as well as others that arose during the course of the Colloquium, found their answer either indirectly in the discussions or directly in other papers presented.

One more point that we permit ourselves to raise is the following. The two areas that are most likely to yield reliable results, both in the domain of morphology and of genetic comparison, are clearly those of the pronoun and the conjugation of the verb. In this we find ourselves in full agreement with, among others, Tucker who concludes his investigation of the problems of genetic relationship within Cushitic with the words: "When one compares the great mass of attested Erythraic languages, one is struck by the overall similarity of the pronominal system and the block and interlocking patterns of the verb conjugation. Consequently, when discussing the genetic relationship of any suspected Erythraic language, these two factors could well be investigated along with vocabulary comparison; and where adequate vocabulary is lacking, these two factors have their own value as providing a useful clue" (1967, 676f). His method differs from ours only in that, while we examined the morphological patterns with a view to tracing the origin of the individual elements and the process by which these patterns were arrived at in the different languages, Tucker focuses his attention on the essential morphemic elements only, consonants or vowels, in the pronominal and verbal prefixes and suffixes. Furthermore, his immediate concern is simply to establish or to refute the Cushitic nature of certain borderline languages which have been either admitted to the group or excluded from it on inadequate grounds.



human beings and most common or domestic animals natural gender is most often expressed through HETERONYMY:

Subgroup	'father'	'mother'	'brother'	'sister'	'son'	'daughter'	'boy'	'girl'
I. Bedawye	baba	de	san	kwa				
II. Agaw	abā, az	ganā	zan	šan	ḥorā			šēgā
Quara				šen				šāgā
III. 'Afar	ábbā	ínā	dān					
Galla	abbā	hāqā						dubrá
Somali	adōg	habar					wīl	gābār
Sidamo	anna	ama						
IV. Janjero	abā	intō/e	aytā	ētnā				
Basketo	bābā	indē	išā	miččē				
Chara	bābi	ini	inisi (iši)	iččil				
Zaisse	awā			indišātō				
Wálamo	aznā <sup>a</sup>	maččiyā/e <sup>b</sup>	iša/e	mičče				
Kaffa	nihō	indē	qabbō <sup>c</sup>	manē				

<sup>a</sup> 'husband'; <sup>b</sup> 'wife'; <sup>c</sup> 'elder brother'.

Heteronymy, although everywhere present, does not affect many nouns and natural gender may also have no formal expression. Male and female beings may thus be designated by EPICENE words or lexemes, e.g. Bedawye 'or 'brother/sister/boy/girl'; Somali *walāl* 'brother/sister'; Sidamo *bēto* 'son/daughter/girl'; *rodó* 'brother/sister'; Ometo *hiriyā* 'comrade (m.f.)', *mūčāyō* 'little child (m.f.)', *sarē* 'dog/bitch'; *bēto* 'son/daughter'. When gender is to be stressed the terms for 'male' or 'female' (Bedawye *rāba* 'male', *kwat* 'female'; Agaw (Kemant) *awrā*, *iyusē*) can be added to the epicene lexeme: e.g. *awrā bagā* 'ram', *iyusē dāmi* 'female cat', *ōsrē giziñ* 'bitch'. Awiya follows the same rule but, besides using the classifier, it adds the feminine marker to substantive and adjective, e.g. *ḥuāna doḥuārā* 'she-ass', *ḥuānā gesānā* 'bitch'. Somali has *lab* 'male', *ḍidig* 'female'; Sidamo has *laba* 'male', *mēa* 'female'. Ometo can have recourse to *adē*, *attūmā* 'male', *mačča* 'female'; in Janjero we find *mašk asu* 'woman' (literally 'female man'), *adk asu* 'man' (literally 'male man').

Tone languages may mark gender through differentiation of tone, which in the case of Cushitic has been recognized as present in, for example, Somali by Armstrong (1934, 116-161), who distinguishes four different tones: high, mid, low and high-falling. This theory has been partly confirmed by Klingenberg (1949, 289-303). Moreno gave some attention to the question of tone (1955, 21 f.) quoting Armstrong and Klingenberg, but reserved final judgment pending further inquiries by Andrzejewski and Galāl (1956, 11 f.) who, eventually, was led to adopt Armstrong's theory. Although well aware that adequate attention ought to be given to the subject, since tone also serves to distinguish meaning in homographs (*gees* 'direction' as against

*gees* 'horn', *laab* 'thorax' as against *laab* 'fold' (imper.), etc.), we shall here limit ourselves to one or two examples, e.g. *inan* 'boy/son', *inán* 'girl/daughter'; *qáalin* 'young he-camel', *qaalín* 'young she-camel'; *islàan* 'old man', *isláan* 'old woman', etc.<sup>3</sup>

As examples of the distinction of gender conveyed through difference of stress we may quote 'Afar *báḷá* 'son', *bālā* 'daughter'; *kútā* 'dog', *kūtá* 'bitch'; *ǫkálō* 'jackass' *ǫkaló* 'she-ass', etc.

When the noun has no mark of gender, as in the case of heteronymy or where a classifier is used, gender can still be made apparent by concord in the article, in demonstratives, or in the suffixes of declension, in the adjective, in the form of the predicate. This is the case in Badawye, which has only one word in which gender is distinguished by means of a grammatical morpheme, namely *tak* 'man', *tak-at* 'woman'. (The article will be treated along with the pronouns and the cases with the morphemes characteristic of the feminine). Agaw *neñ* 'house', masculine, takes the feminine form of the adjective when 'little, small' is added: thus *šeguē neñ* (-ē being the feminine ending); Galla *andanqō guddā* 'a big cock', *andanqō guddō* 'a big hen'.

#### *Grammatical or morphological gender*

##### *Gender in the noun-adjective*

I. BEDAWYE as mentioned above, marks gender by means of a special morpheme in one word only, *tak* 'man', *tak-at* 'woman'. Indefinite nouns (i.e. without article) ending in a vowel can, however, take *-b* for masculine, *-t* for feminine in the accusative case with lengthening of a previous vowel. In Tu-Bedawye *-b* can occur also in the nominative and *-t* occurs both in the nominative and the accusative, e.g. *áne šā-t tamanyēk* 'wenn ich Fleisch esse' (Reinisch 1893-1894, § 122d, 66 f.). Sometimes *-t* can be added when the noun is supplied with the article: *t-hawā-t* (for *tō-hawā-t*) *tetīb* 'sie füllte den Schlauch an' (Reinisch 1893-1894, 67). If a feminine noun is qualified by an adjective, only the adjective takes the object feminine marker *-t*, e.g. *daūrī-t* 'ōr 'a nice girl'. The genitive is obtained by placing rectum plus *-i* m., *-ti* f., before regens ('ōr-*i* 'ōr 'Sohnes Sohn, Enkel'; *abā-ti derág* 'Ufer eines Flusses'). If the rectum is furnished with the article, this is placed in the accusative (*ō-gāw-i kīna* 'Herr des Hauses'). If the rectum is a plural it takes *-y* m., *-ti* f., after the plural ending. The *i* in *-ti* is the genitive *i* and to be compared with *-i* of Bilin, Khamir, Saho-'Afar. In Tu-Bedawye when both rectum and regens are feminine the rectum takes *-tīt*, double feminine: 'ó-*tīt* (for 'or-*t-it*) *de* 'a girl's mother' (Roper 1928, § 49, 14). When the adjective is used as a predicate it receives the copula (substantive verb) as suffix, and if it ends in a vowel the connection is made by means of *-b* m. and *-t* f. and the

<sup>3</sup> For the general question of tone languages in Africa cf. Westermann and Ward (1957), ch. 24, 133-157, with bibliography. On 'tone' in contrast to 'intonation' and 'stress', see Abercrombie (1967), 104ff.

vowel is lengthened: *anī daūrī-b-u*, *e* 'I (m.) am nice', *anī daūrī-t-u*, *e* 'Id.' (f.) (Reinisch 1893-1894, § 139f., 82f.).

II. AGAW can show feminine gender by means of two different suffixes:

(1) *-t* for nouns, in Kemant (*kəvan-anti* 'fecund woman'). This is rather rare and is also found in Bilin and Awiya (the two extreme branches of Agaw), but the latter has developed it more than the former (Bilin *dān* 'brother', pl. *jān*, f. *jān-t* 'sister').

(1a) *-ā*, normal use in Awiya (*agal* 'master', *agal-ā* 'mistress'; *jimanti* 'male dancer', *jimant-ā* 'female dancer'; *gesānī* 'dog', *gesān-ā* 'bitch').

(2) *-ti*, clear examples of which are found in Dembia (*dirwā* 'cock', *dirwa-ti* 'hen'; *ger* 'bull calf', *gel-ti* 'female calf'). Kemant has *yil-t* 'eye' (cf. Egyptian *ir-t*) as opposed to Dembia, Quara *yil*, Khamir *yel*, *el*, which are masculine.

(2a) *t-* as prefix (Bilin *t-eğr-i*, Quara *t-ayr-i* 'aunt'; Bilin *t-anšin-i* 'mother in law', *anšin* 'father in law'; Bilin, Quara *t-adārā*, Kemant *yitatarā* 'lady, dame', literally 'my lady').

Adjectives in Agaw are of three types:

(a) primitive, found in Bilin (North) and Awiya (South). They form their feminine in *-i*, cf. nouns (*jikaw* 'heavy', f. *jikaw-i*);

(b) relative, in *-ō* (Kemant), in *-ō*, *-ow* (Awiya), in *-āw*, f. *-ē*, pl. *-ō* (Quara), in *-aw*, f. *-ray*, pl. *-awk* (Khamir), in *-awh*, f. *-rari*, pl. *-aw* (Bilin). No feminine or plural forms are attested in Kemant (Conti Rossini 1912, 136).

(c) denominatives, in *-ū* (Kemant), in *-ū*, f. *-ē*, pl. *-ū/-w* (Quara), in *-ū*, f. *-i*, pl. *-uk* (Khamir), in *-uh*, f. *-ri/-di*, pl. *-ū/-uw* (Bilin).

III. 'AFAR. Feminine gender is here expressed by means of vowel suffixes. All nouns ending in *-ā*, *-ē*, *-i*, *-ō*, *-ū* are feminine, all others are masculine. Gender is also marked in the 'individual' form of the noun; thus, while *lūbāk* means 'lion' in general as a class and forms a plural *lūbuk* 'lions', an individual of the class is indicated by means of a suffix (*-y*)-*ta/to* which, to mark the feminine, receives the accent (*lūbāk-tō* 'a single lion, he-lion', f. *lubāk-tō* 'a single lioness'). The plural for masculine and feminine is *-tit*. The *-y-ta/to* form of the suffix is used with nouns ending in one of the vowels *a*, *e*, *o*, *u* (*qkalō-y-tā* 'ass', f. *qkalō-y-tā*, from the class noun *qkalo*, f. *qkalō*, plural *qkalol*).

Gender appears, indirectly at least, in the verbal (abstract) nouns: root plus *-t/to* (*amana* 'to defend', *amanā-t* 'protection', pl. *-ot*; *rahasa* 'to be rich', *rahasā-t* 'riches',

pl. *rahás-ot*. For *-to*: *kahana* 'to love', *kahan-tō* 'love'; *rahas-tō* 'riches'). Nomina agentis are formed by means of the suffix *-tū*, f. *-tū*, pl. *-tīt* (*harasa* 'to plough', *harás-tū* 'ploughman', f. *haras-tū*, pl. *harás-tīt*; *kataba* 'to write', *katáb-tū* 'scribe', f. *katab-tū*, pl. *katáb-tīt*; Colizza 1887, 37 f.).

GALLA. In the noun we must distinguish an 'absolute' or 'primary' form, with no distinction of gender and ending in a vowel *ē*, *ī*, *ū*, *ō*, *ā* and the consonant *n*. The absolute form serves as direct object and vocative, and from it is derived the subject form by means of a suffix *-n(i)* (whether *n* or *ni* is chosen is governed by euphony): *sarē* 'dog', subject *sarēn(i)*; *dubbī* 'word', *dubbīn(i)*; *nāmā* 'man', *nāmni*; *wāqā* 'heaven, God', *wāqni*, etc. (note that *ā*, *ē*, *ī*, *ō*, *ū* = *á*, *é*, *í*, *ó*, *ú*). Another suffix for the formation of the subject with nouns in *-ā* is *-ti* (*lafā* 'earth', *laf-ti*, the *-ā* falling; *intālā* 'daughter', *intāl-ti* or *intālli* < \**intal-ni*; *biyā* 'earth', *bī-ti* < \**biy-ti*, as well as *bī-ni* and *bīyi*).

With regard to the problem under discussion, it is interesting to note that *-ti* is "an old feminine termination, now also used for the masculine" (Moreno 1939, 37). This same ending is much more frequent in southern Galla for nouns with terminations other than *-ā* (*nītī* 'woman, wife', subject *nītī-ti* or *nītī-n*; *rē'ē*, *rē* 'goat', subject *rē-ti*). Both suffixes *-ni* and *-ti* may be used together (*laf-tī-n*, *bī-tī-n*). Finally, nouns in *-ā* mostly with derivational suffixes *-ēsā* (*-ēččā* Shoa) and *-eñā*, can form the subject by changing *ā* to *i* (*sor-ēsā*, *-ēččā* 'rich, notable', subject *sorēsī*, *sorēččī*; *obolēsā*, *-ēččā* 'brother', *obolēsī*, *-ēččī*; etc.).

Adjectives and substantives of adjectival derivation can have a feminine in *-ti* (*sorēsā* 'the rich', *soretti* 'the rich woman'; *mōti*, *mōtiččā* 'king', *mōtitti* 'queen'). Analogically the masculine ending *-áččā* changes into *-atti* and *-iččā* into *-itti* (*dallaččā* 'reddish', f. *dallātti*; *furdiččā* 'big', f. *furditti*). The element *-t* as feminine marker also occurs in the suffix *-tu* with nouns in *-ā* (*dīmā* 'red', f. *dīm-tu*; *hamā* 'bad', f. *hām-tu*; *barēdā* 'beautiful', f. *barēd-du*, with *t* becoming *d*; etc.).

Another type of feminine is met with in nouns of the same formation, i.e. in *-ā*. Some of these change *-ā* into *-ō* for the feminine (*guddā* 'large', f. *guddō*; *ṭinnā* 'small', f. *ṭinnō*, pl. f. *ṭiṭinnō*; *ṭiqqā* 'little', f. *ṭiqqō*; *furdā* 'big', f. *furdō* (see above: *furdiččā*).

Adjectives ending in *-ē*, *-i*, *-o*, *-u* have one form only for both genders.

In addition to the occurrences listed above the *-ti* element can serve yet other purposes. It may enter into the formation of the plural *-ōt-ā* (*bartu* 'disciple', pl. *bart-ōt-ā*; *hama* 'bad', pl. *ham-ōt-ā* 'the bad [people]'). It may also express the idea of possession (*harre-ko-ti* 'my ass'), mark the indirect object and the locative.

SOMALI. In a few nouns gender is indicated by the derivational suffix; thus are masculine abstract nouns in *-is* (*weyd-is* 'interrogation') and are feminine abstract derivatives in *-ān*, *-in*, *-nin*, *-nimo* (*badn-ān* 'multitude'; *fūr-an* 'act of opening'; *naḍif-in* 'cleanliness'; *sūg-nin* 'hope'; *wālal-nimo* 'brotherhood'). The same applies in the case of Arabic loanwords (*fā'ido* 'earning' from Arabic *fā'ida*; *sāno/-a* 'year' from Ar. *sana*<sup>t</sup>). Nomina actionis in *-id* are feminine (*šēg-id* 'saying'), those in *-ów*

(-o) are masculine (*fur-ów* 'opening'; *q̄is-ów* 'building'; *absad-ów* 'fearing'). These are characteristic of Benâdir.

Adjectives, as stated above, have only one form for both genders. Exceptions are the adjectives with suffix *-lów*, which changes into *-léy* for the feminine (*bēn-lów* 'liar', f. *bēn-léy*).

The various plural forms are affected by the phenomenon of 'polarity' to which we shall return later.

SIDAMO is poor in morphological markers of gender. As we have seen, natural gender is expressed by heteronymy or by epicene terms or indicated by the verbal endings or by means of *laba* 'male', *mēa* 'female'. Morphologically the distinction of sexes can be marked through contrasting suffixes:

(a) *-ō* m., *-ē* f. (*ar-ō* 'husband', *ar-ē* 'wife, woman'; also *dančā* 'beautiful, good', f. *danč-ē*). The same applies in Janjero, Walamo and Gofa (cf. *-i* of Agaw above). But these are rare examples, according to Cerulli (1938a, 118 f.). A remnant of a *-t* element might be preserved in *mēn-t-ō* 'woman' as opposed to *mannō* (individual *mančō*) 'man', but the feminine form is not easily explained (1938, 118). Natural gender is indicated by the presence of the demonstrative pronouns and the verbal endings (see below).

(b) *-te*, corresponding to Galla *-ti* (*elo* 'lover', f. *elō-te*; *fantiččo* 'servant', f. *fanti-te*; *sorēsā* 'rich', f. *sorētte*; *motiččo* 'sir', f. *motitte* 'lady', cf. Galla). The same suffix is used, although rarely, for adjectives: *-ta/-te*, subject case *-te*, *-ti* (*lowō* 'grown up', f. *lowōtti*; Moreno 1939, 22 f., 30 f.).

IV. JANJERO is even poorer than Sidamo in morphological affixes, the only remnant of a feminine ending being *-ē* in *asus-ē* 'woman' contrasted with *as-ā* 'man'. Nor does it have a plural marker for nouns (Cerulli 1938b, 52 f.).

OMETO has morphological markers of gender for a few nouns only: *-ya* (*-e/-o*) and *iye*, which can become *ye*, *je*, *ē* (*asā* 'man', *as-iya/e* 'woman'; *na'ā* 'son', *na'iye* 'daughter', *nā'ē-s* from *na'-iye* 'to the daughter').

KAFFA has preserved the feminine morpheme *-ē* as found in other languages of the group (Janjero, Ometo) but here it has a wider diffusion (*ūrō* 'man', f. *ūr-ē* 'woman'; *bakkō* 'chicken', f. *bakk-ē* 'hen'; *gēnō* 'old man', f. *gēn-ē* 'old woman'; *bušō* 'boy, son', f. *buš-ē* 'girl, daughter'; *manō* 'brother', f. *man-ē* 'sister'; cf. also *mačč-ē* 'woman', *indē* 'mother'. This suffix *-ē* for feminine, contrasting with masculine *-ō*, is characteristic of the Kaffa language. According to Cerulli (1951, 302 f.) Kaffa has here preserved an ancient trait of general Cushitic.<sup>4</sup>

<sup>4</sup> Greenberg's statement (1955, 48) that gender is lacking in the languages of the Western subgroup (Kaffa, etc.) is to be corrected accordingly.

*Gender in the pronouns*

A general observation applicable to the whole Cushitic family is that no distinction of gender is found in the plural, which has a common form for masculine and feminine. In the singular gender finds expression only in the third person, with a very few exceptions in which masculine and feminine are marked also in other persons. The very notable exception to this rule is Bedawye.

I. BEDAWYE. The independent personal pronoun is made up of three component elements:

- (a) a common prefix *ba-* for second and third persons singular and plural;
- (b) an internal element *r/t* marking masculine and feminine for second and third persons respectively;
- (c) a termination *-ūk/-ūs* for the second and third persons singular, *-kn-a/-sen-a* for the second and third persons plural. First singular and first plural are built on a different pattern (*an-i-h* sg., Beni Amer *anī, anī*; *han-án* pl.) in agreement with Western Semitic.<sup>5</sup> It is immediately apparent that Bedawye has a twofold way of marking gender in both singular and plural, one by means of the alternance of an internal element *r/t*, and the other by means of the ending *k/s*.

TU-BEDAWYE preserves the alternant *r/t* but has dropped the third element in the third persons singular and plural (Roper 1928, § 80, 24).

The same pattern in the distinction of gender is shown by the possessive pronoun. The consonantal elements are the same but *-y/-i-* is inserted between *r/t* and the termination (*bar-y-ūk/bat-y-ūk* etc. and *bar-i-tūk/bat-i-tūk*, with a masculine and feminine noun respectively). The objective form shows the alternance of the vowels *ō/ē* (for *ū/ā*) respectively (2nd sg. *bā-r-ōk*, pl. *ba-r-ēk[na]*).

The suffix form of the possessive pronoun repeats the pattern with few modifications and, of course, with the dropping of the first component element *bar*. One notable feature is the appearance in the second singular of a new opposition to mark the gender of the possessive: m. *-ka*, f. *-ki* (corresponding exactly to Semitic *-ka, -ki*). But the simpler form *-(y)ūk* for masculine and feminine also occurs and it is the only one attested in Tu-Bedawye (Roper 1928, § 102, 30).

Normally a noun with the possessive suffix is preceded by the article which marks gender both in the singular and in the plural:

nom. sg. m. *wū, ū*, f. *tū*; pl. m. *yā, ā*, f. *tā*  
 acc. sg. m. *wō, ō*, f. *tō*; pl. m. *yē [yī]*, *ē*, f. *tē [te]*.

<sup>5</sup> For the analysis and discussion of the forms see Castellino (1962), 12f.

The *-t* of the feminine article is suffixed to numerals when these are followed by feminine substantives.

Gender is again marked in the verbal suffix pronoun, but in the singular only in the first and second persons and not at all in the plural:

sg. 1st c. <i>-hēb</i> , <i>-hib</i>	2nd c. <i>-hōk</i>	3rd c. <i>-hōs</i>
m. <i>-hēba</i>	m. <i>-hōka</i>	
f. <i>-hēbi</i>	f. <i>-hōki</i>	
pl. 1st c. <i>-hōn</i>	c. <i>-hōk-na</i>	c. <i>-hōs-na</i>

Here *b/k/s* mark the first, second and third persons respectively, in the singular and in the plural, while the opposition *a/i* marks the gender. In Tu-Bedawye, where these forms occur, *-a* and *-i* indicate "the sex of the person addressed" (Roper 1928, § 98, 29).

The demonstrative pronoun 'this' is essentially made up of the article plus the characteristic ending *-n*:

subject m. *ūn*, pl. *ān*; f. *tūn*, pl. *tān*  
 object m. *ōn*, pl. *ēn*; f. *tōn*, pl. *tēn*

The marker *-t* for the feminine occurs again in a different position in the demonstrative for remote things:

subject nom. sg. m. *bēn*, pl. *balīn*; f. *bēt*, pl. *balīt*  
 object nom. sg. m. *bēb*, pl. *balīb*; f. *bēt*, pl. *balīt*.

From the article is also formed the indefinite pronoun in Tu-Bedawye: m. *kissā* (*kassā*), f. *kistā* 'all, every' (Roper 1928, § 115 ff., 36).

Gender can also be marked in interrogative pronouns, as m. *au*, f. *aut* 'who?'; gen, m. *āi*, f. *ait* 'whose?', etc.; m. *nā*, f. *nāt* 'what?'. These can appear in combination with other elements.

II. AGAW is far less rich in the expression of gender in the pronominal system. Bilin, the most archaic and pure language of the group,<sup>6</sup> opposes gender in the personal pronouns only in the third person singular: m. *nī*, f. *nirī*. Khamir opposes m. *ieñ*, *ñi*, f. *ñir*; Khamta has m. *nu*, f. *ñinč* (with f. *kit* as against m. *ketā* for the second person, the only exception in the group); Kemant has m. *nī*, f. *niy*; Damot has m. *nā*, f. *ñi*. Quara and Awiya do not distinguish gender in the personal pronoun.

In the objective form of the pronoun the distinction of gender is greatly reduced

<sup>6</sup> Cf. Conti Rossini (1912), 27f.; note however that the archaic character of Bilin was contested at the Colloquium.



and the third person feminine *ku-* of Kemant is considered "très douteuse" (Conti Rossini 1912, 45). As for the other languages, Bilin and Khamir oppose m. *nī* to f. *nir*, but Khamir uses the explosive *ṇ*; Quara has m. *nī*, f. *niš*. These forms of the object pronoun, when prefixed to a noun, also serve as possessive. The form of the object pronoun employed as a suffix is not clearly attested in Agaw. Only Bilin and Khamir can have the suffix, but the noun must be reinforced by means of apocopated forms of the substantive verb, with the effect that the pronoun is then practically transformed into a relative clause (Khamir *yen firzān y-ū yēh* 'hic equus meus est', literally 'hic equus est qui meus est'; Conti Rossini 1912, 47).

In the demonstrative pronouns, only Bilin has the independent secondary form for feminine *nirīn* 'this', beside the common form *inā* for singular and plural; *injəhan-nirī* 'that', alongside *injā* for masculine and feminine, singular and plural. Khamta opposes m. *enčə*, *ənil* 'this' to f. *ənič*, m. *nā* 'that' to f. *net*; Awiya opposes m. *en* 'this' to f. *ennā*, m. *an* 'that' to f. *anā*; lastly, Damot has respectively m. *enni* 'this', f. *nāw(?)*; m. *aneñen(?)*, 'that', f. *anā*.

III. SAHO-'AFAR. Like the other languages of the group, it distinguishes gender in the personal pronouns only in the third person singular: m. *ússyk*, *ussykū*, f. *issī*, *iss*, *iššī*.

GALLA has m. *in-nī*, f. *išin*, *išēn*, *isīn*.

SOMALI. Short forms: m. *ās/ūs*, *āu/āu*, f. *āy*; medium forms: m. *ussu*, f. *iy-yo* (from *āy-yo*); emphatic forms: m. *u/assu-ga*, *ussi-ga*, f. *i/ayya-da*, *iyye-da*, *īda*.

HADYA: m. *iṭṭē*, *iṭṭi*, *iṭ*; f. *isē*;

KAMBATTA: m. *inni*, *isu*, f. *isē(-tānnē)*;

SIDAMO: m. *isi*, *isó*, f. *isē*;

BURGI: *isi*, f. *iše*.

In Low Cushitic the objective, the possessive (suffixed) and the demonstrative pronouns show the alternance *k/t* for masculine and feminine respectively.<sup>7</sup>

SAHO-'AFAR: objective pronoun 3rd m. *kāy*, *kā*, *kāyā*; f. *tāy*, *tē* (from *ta-ay*), *tā*; dative *-ak(ā)*.

SOMALI. There is no distinction of gender in the personal objective pronouns: *i/kū/ū*, pl. *na/idin/ū* for first, second and third respectively. The possessive pronouns show the alternance *k/t*: for masculine nouns 3rd m. *kīs*, f. *kēd*; for feminine nouns 3rd m. *tīs*, f. *tēd*. A parallel set is formed with the same elements (which are those of the article) plus *-(g)a* m. and *-(d)a* f.

GALLA forms the objective personal pronouns with an element *-s* for second and third persons, plus a vowel opposition *a/i* for third person masculine and feminine: 1st *anā*, 2nd *sī*; 3rd m. *isā*, f. *išī/ē*, *isī*. The objective plural agrees with the subjective.

The possessive adjectives, however, show the same alternance of *k/t* as is found

<sup>7</sup> For the presence of the *k/t* opposition in the Chad languages see Greenberg (1955), 49.2 and, for the wider area of Northern Africa, Tucker and Bryan (1966), 22-25,



in Saho-ʿAfar and Somali: *ko*, *kīya*, *kē*; *to*, *tīya*, *tē*; the plural is common to both sets: *sa*, *šī/ē*, *sani*.

Sidamo has the same form for the personal objective suffix pronoun and the possessive: 3rd sg. m. *sī*, f. *se*.

In the demonstratives, however, we again have *k/t*: Galla, subj. *kun(i)/tun(i)*, pl. *kani/tani*; *sun(i)/sana*, pl. *sani*; obj. *kana/tana*, pl. *sani*.

SIDAMO. The pattern is less regular than in Galla; 'this' m. *ko*, *ku*, *konne*, *kun(n)i*, f. *te*, (*at*)*tenne*, *tēne*, *tin(n)i*, *tin*; 'that' m. *ha-kko* (*y/ye*), f. (*h*)*a-tto(-i, -enne, -ēne)*, *atti* but also m., f. *kū-ri*, *ku-ri-ri* and *ko-re*, *ko-rē-re* (*-ri/-re* = pl.).

In addition to Sidamo, the long forms are found in Kambatta and Darasa; Hadya, on the contrary, has the shorter ones.

The article (a former demonstrative), which is also used as a relative, opposes m. *-ha/u*, *-ho* (< *\*ka*, *\*ku*), pl. *-re/i* (also functioning as collective and neuter) to f. *ta/e* (obj.), *ti/e* (subj.), cf. Kambatta *tānnē*. For the suffix forms cf. Burgi m. *-ka* (*-ga*), f. *-ta* (*-da*), *\*ti* > *ši*; Darasa m. *-ke* (*-ka*), f. *-te/-ta*; Hadya m. *-ka*, f. *-ta*; Somali m. *-ka/kiku*, f. *-ta/tittu*, where the vowel alternance serves to mark the physical or appreciative position of the person spoken to in relation to the speaker. The article plus *-n* yields the demonstrative adjectives *kān/tān*.

The *k/t* opposition is finally found in some interrogative pronouns: Somali *kē* (< *\*ka-ay*), *tē* (< *\*ta-ay*); Sidamo m. *hi-kko*, f. *hi-tte(nne)* 'which? what?'. Cf. also the interrogative adverbs: *hi-kko* 'where?', *hi-tto* 'how?'.

Indefinite pronouns. Saho-ʿAfar opposes, from the point of view of gender, m. *tīyā* to f. *tīyā* 'one, only' and from this are formed compounds by prefixing *wīl* 'one' or *umān-* 'every, any'. Another pronoun is *aggidāytō*, f. *-tō* 'somebody'. Galla opposes m. *tokko* to f. *takka* 'one' which, when reduplicated, mean 'someone'. Sidamo has a suffix *-nka/u/i* for masculine and *-nta/e/i* for feminine as a generalizer, used with the noun and the verb when this is interpreted as a relative (Moreno 1940, 39).

IV. JANJERO has no distinction of gender for any person, singular or plural; thus *bar*, *ham-bar* serves both for masculine and feminine (see Cerulli 1938b, 11 f. for etymologies of the form).

#### *Gender in the verb*

How does gender find expression in the verb, the most important part of speech? In the languages of the Indo-European family, gender as such is absent from the verbal finite forms and only appears, as is natural, in the verbal nouns and participles. In Semitic, on the contrary, gender finds expression also in the verb and the same is true of Cushitic. Given the limitations imposed by a paper of this type and the mass of material to be dealt with, it is not possible to give an exhaustive treatment of the

topic; we will therefore consider outside our scope the presentation of general questions concerning the Cushitic conjugation. To give a very broad idea of the situation it is sufficient to point out that some languages of the group (Bedawye, Saho-'Afar, Somali, Agaw) have two different types of conjugation: strong and weak. The former makes use of prefixes and suffixes, with alternation of the characteristic vowel, whereas the latter makes use of suffixes only, the root remaining unchanged. Other languages of the group on the other hand (Galla, Sidamo, Ometo, Kaffa, etc.) have only the weak conjugation. Both sub-groups, in addition to the basic conjugation, can form 'derived' conjugations similar to those we know in Semitic (Western and Eastern, i.e. Akkadian). Verbs can be primitive or denominative and are supported by a small number of auxiliaries, as is found also in the Semitic language Amharic. The tenses are fundamentally two in number, imperfective and perfective, plus a relative or subjunctive with parallel formations (short and long forms). The moods show a larger development than in Semitic. Alongside the positive conjugation there is a negative one throughout Cushitic. The root is normally of the pattern CVC or VC, with a percentage of tri-literal roots most of which are loans from Arabic. (Regarding all these questions we refer the reader to Castellino 1962, p. II and the bibliography given there).

Gender is marked in the verb only in the third person singular, with rare exceptions (e.g. Sidamo, where gender also affects the first and second persons singular and the first person plural). Methodologically it would seem appropriate to start our analysis with the weak conjugation of Saho-'Afar, a member of the Eastern sub-group. The conjugation appears in its simplest form as root plus terminations (which consist of pronominal elements or forms of the auxiliary *a* 'to say'). The pattern (I) for the imperfect is: 1st c. *-a*, 2nd c. *-ta*, 3rd m. *-a*, 3rd f. *-ta*.<sup>8</sup> The plural, in which there is no distinction of gender, consists of the same elements plus the plural marker *n*. Thus for the verb *ab* 'do, make', the conjugation is as follows: sg. 1st c. *áb-a*, 2nd c. *áb-ta*, 3rd m. *áb-a*, 3rd f. *áb-ta*; pl. 1st. c. *áb-na*, 2nd c. *áb-tan*, 3rd c. *áb-an*.

The same pattern in the singular and plural, with change of vowel *a* to *ä* (*ä*), is employed to form the perfect tense. The formal identity between the first person and the third person masculine on the one hand and the second person and the third person feminine on the other may appear somewhat strange. Whatever the etymology and original meaning of these elements, there can be no doubt as to their present functions. Thus, with only two morphemes are obtained four semantemes. The explanation may be as follows. The system would appear to work in a twofold opposition: (1) an 'I' and 'thou' opposition, employed in dialogue when a person addresses another person; (2) a second opposition, analogical to that which is found in the noun where gender plus number may 'cross' in distinguishing and marking the outside reality according to the category of *minus* as opposed to *maius* or *potior* to *minor*.

<sup>8</sup> For its origin, and connections with Semitic, see Castellino (1962), 8f.; Cerulli (1938a), 105-113.

The same pattern also occurs for the conjugation of weak (ordinary) verbs in Somali: 1st c. and 3rd m. 'un-*a*, 2nd c. and 3rd f. 'un-*ta*. Semantically this expresses the *habitualis* imperfect; for the perfect, -*i* is added to the elements to give -*ay* (from -*a-i*), -*tay* (from -*ta-i*). The relative form is obtained by changing *a* to *o* to give -*o/-to*; other moods repeat the same pattern and make use of special particles.

Together with Saho-'Afar and Somali can be grouped Sidamo. In Sidamo the simpler form uses the pattern as already described. The 'enlarged conjugation' combines the simple conjugation, marking the person only in the first and second singular and person and gender in the third, with auxiliary elements *m/t/n* for the three persons to which are added vowel terminations -*o/-a* marking gender in the first and second singular and the first plural. Note that in the third singular the distinction of gender is marked by the element of the simpler form (*a*, *ta*), while in the plural gender is marked only in the first person. A comprehensive formula for both tenses would then be:

	simple conjugation (imperfect or perfect)	auxiliary element	gender vowel m.    f.
root +	1st c. $\left\{ \begin{array}{l} -e \\ -[t]a \end{array} \right\}$	$\left\{ \begin{array}{l} -m- \\ -t- \end{array} \right\}$	$\left\{ \begin{array}{l} -o \quad -a \\ -o \quad -a \end{array} \right\}$
	2nd c. $\left\{ \begin{array}{l} -[t]a \\ -a \end{array} \right\}$	$\left\{ \begin{array}{l} -t- \\ -n- \end{array} \right\}$	$\left\{ \begin{array}{l} -o \quad -a \\ -o \quad -a \end{array} \right\}$
	3rd m. $\left\{ \begin{array}{l} -a \\ -ta \end{array} \right\}$	$\left\{ \begin{array}{l} -n- \\ -n- \end{array} \right\}$	$\left\{ \begin{array}{l} -o \\ -o \end{array} \right\}$
	3rd f. $\left\{ \begin{array}{l} -ta \\ -ti \end{array} \right\}$	$\left\{ \begin{array}{l} -n- \\ -n- \end{array} \right\}$	$\left\{ \begin{array}{l} -o \\ -o \end{array} \right\}$

Note the *e* (for *a*) in the first person and *a* (for *ta*) in the second person. The latter can be explained as a simplification of -*ta*, -*to*, the auxiliary element assuming the additional function of person marker (see Cohen 1927, 169-200, esp. 169, and Cerulli 1938a, 106 f. who discusses the facts for all the Sidama group where the pattern is not so regular). The auxiliary element usually appears doubled, but forms with simple consonant do also occur. The distinction of gender in first and second persons is not always observed (Moreno 1940, 44 f.). Secondary forms or variants for both tenses are not considered here.

It may be convenient to set down the paradigm of the simple and of the enlarged conjugations for the verb *hun-a* 'to exterminate' (taken from Moreno 1940, 44 f.):

simple conjugation		enlarged conjugation	
		imperfect	perfect
sg. 1st c.	<i>hun-a</i>	1st m. <i>hun-é-mm-o</i>	1st m. <i>hun-ó-mm-o</i>
		1st f. <i>hun-é-mm-a</i>	1st f. <i>hun-ó-mm-a</i>
2nd c.	<i>hun-ta</i>	2nd m. <i>hun-á-tt-o</i>	2nd m. <i>hun-ó-tt-o</i>
		2nd f. <i>hun-á-tt-a</i>	2nd f. <i>hun-ó-tt-a</i>
3rd m.	<i>hun-a</i>	3rd m. <i>hun-á-nn-o</i>	3rd m. <i>hún-i-n-o</i>
3rd f.	<i>hun-ta</i>	3rd f. <i>hun-tá-nn-o</i>	3rd f. <i>hún-ti-n-o</i>

pl. 1st c.	<i>hun-na</i>	1st m.	<i>hun-n-é-mm-o</i>	1st m.	<i>hun-n-ó-mm-o</i>
		1st f.	<i>hun-n-é-mm-a</i>	1st f.	<i>hun-n-ó-mm-a</i>
2nd c.	<i>hun-tina</i>	2nd c.	<i>hun-ti-n-á-nn-i</i>	2nd c.	<i>hun-ti-n-ó-nn-i</i>
3rd c.	<i>hun-na</i>	3rd c.	<i>hun-n-á-nn-i</i>	3rd c.	<i>hun-n-ó-nn-i</i>

A slight variation of type I (type Ia) is shown by Galla: sg. 1st c. *-a*, 2nd c. *-ta*, 3rd m. *-a*, 3rd f. *-ti*; pl. 1st c. *-na*, 2nd c. *-tan(i)* or *-tu*, 3rd c. *-an(i)* or *-u*. Thus, for the singular of *bēk* 'to know': 1st c. *bēk-a*, 2nd c. *bēk-ta*, 3rd m. *bēk-a*, 3rd f. *bēk-ti*. In the perfect *a* is replaced by *ē* and in the subjunctive by *u* in all the singular forms and in pl. 1st c. (cf. Akkadian).

Agaw, while essentially preserving the pattern and agreeing with Galla with regard to the third person feminine, (*a*-)*ti*, has two variations, one affecting the second person and the other the composition of the verbal stem. The second person, instead of *-ta*, has *-ra* in Bilin and Khamir and *-ya* in Kemant and Quara. But *-r* in Agaw represents an old *-t*-, as does the *y* of Quara (cf. Conti Rossini 1912, 68),<sup>9</sup> so that the pattern is only phonetically changed. A new feature is found in a third component, which follows the pronominal elements, *-ku* for first to third masculine (Kemant and Quara), *-kun* (first), *-uk* (second and third) in Bilin and Khamir.<sup>10</sup> Of more interest for us is the variation in the suffix of the third person feminine, *-a-ti*, which is common to all the sub-group (Khamir (*-a*)*č* is from *-ti*). This (*a*)*ti* of Galla and Agaw introduces a new distinction between masculine and feminine. The system, which consisted of two elements, now has three *a*, *ta*, *ti* and as a result in the third person gender receives independent expression. In Bilin, for example, the conjugational elements are therefore: 1st c. *-a-kun*, 2nd c. *-ra-uk*, 3rd m. *a-uk*, 3rd f. *-a-ti*.

Somewhat different is pattern II which occurs in the weak conjugation of Bedawye: sg. 1st c. *an*, 2nd m. *ta*, 2nd f. *ta-y*, 3rd m. *ya*, 3rd f. *ta*; pl. 1st c. *na*, 2nd c. *tā-na*, 3rd c. *yā-na*. Gender and person are here distinguished in the second and third persons by a special morpheme *i* which in the second person marks the feminine (*ta-i*, giving *tay*), and in the third persons singular and plural marks the masculine (*i-a*, giving *ya*). (By positing the morpheme *i* in *ya* we do not mean that this is the etymology of *ya*).

Pattern II occurs in the weak perfect described by Reinisch (1893-1894, 178, 183 f.) and the "past tense positive" of Tu-Bedawye (Roper 1928, 40 f.), but it represents the ancient imperfective (Moreno 1940, 297). The present positive is formed by means of an extension, *ni/e*, originally a demonstrative, which follows the pronominal elements and has accentuating character (cf. Darasa *-ni*, Burgi *-na*, in Moreno

<sup>9</sup> An analogous change occurs in Egyptian where final *r* passes to *j*, the grapheme for *r* either being preserved alongside the grapheme for *j* or omitted altogether: *bnr* 'date fruit', later written *bnj*; *mr*, later *mj*, 'as'.

<sup>10</sup> For the analysis of this feature see Reinisch (1909), 2f.; Conti Rossini (1912), 69-72; Castellino (1962), 6ff.

1940, 299 f. For Roper these terminations agree with the past indicative of the verb *di* 'to say' (§ 251) to which he refers in § 131).

As regards sub-group IV, Omo, we shall limit ourselves to two observations. Firstly, the conjugational pattern in these languages is not so clear cut as the one we have been analyzing. Compared with this latter it shows only traces that link the two, and therefore its interest for us is greatly diminished. In the second place, where distinction of gender is detectable in this pattern (that is in the third person singular, as is normal in Cushitic), the morphemes used to express it differ noticeably from those of the other groups: "Questo tipo protocuscitico di coniugazione (our types I and Ia) è stato profondamente alterato dall'Ometo e dal Caffino nel quale non se ne ritrovano che poche tracce".<sup>11</sup>

Turning now to the second conjugation, that of the strong verbs, we find something that cannot fail to cause us some surprise. For, although the strong conjugation is obtained by PREFIXING the pronominal elements to the verbal root, these elements are none other than those of the suffix conjugation and have practically the forms found in Bedawye: 1st c. *a*, 2nd c. *ta[y]*, 3rd m. *ya*, 3rd f. *ta*. Thus, in Saho-'Afar *abba* 'to listen, hear' conjugates: sg. 1st *á-bbā*, 2nd *tá-bbā*, 3rd m. *yá-bbā*, 3rd f. *tá-bbā*; pl. 1st *na-bba(n)*, 2nd *ta-bba(n)*, 3rd *ya-bba(n)*. Note the alteration of the vowel *a* to *ā* and the absorption by the prefix of the vowel of the verbal root (cf. *kata* 'to collect, gather': *á-kā-tā*, *ta-kā-tā*, etc.). The perfect has *o* or *u* as the vowel of the prefix.

Somali reproduces the same pattern for its strong verbs (five in number): e.g. *qān* 'to know': sg. 1st c. *a-qān*, 2nd c. *ta-qān*, etc., pl. 1st c. *na-*, 2nd c. *ta-*, 3rd c. *ya-qān*; perfective sg. 1st c. *a-qīn-ey* (*aqīnni*), 2nd c. *ta-qīn-ey*, etc., pl. 1st *na-qīn-ey*, but 2nd *ta-qīn-ēn*, 3rd *ya-qīn-ēn*. While Benadir has this pattern, Isaq behaves differently having changed *a* to *i*, thus 1st *i*, 2nd *ti*, 3rd m. *yi*, 3rd f. *ti*. The other strong verbs are *āl* 'to stay', *mād* (< *māt*) 'to come', *ḍah* 'to say', *hay* 'to be'. Somali (Darôd), however, has hybrid forms, some verbs being conjugated both with prefixes and suffixes, and there is no alternation in the root vowel.

We must leave aside the whole Sidamo sub-group since it has no strong conjugation, as well as the Omo sub-group for the same reason. Thus only Bedawye is left for us to analyze.

Bedawye not only has a prefixing, or strong, conjugation but it is the only language in the Cushitic group to show a large measure of agreement with Semitic (and especially with Eastern Semitic). Strong verbs are divided into two classes: monosyllabic and bisyllabic. The monosyllabic verbs have *i* and *ā* respectively for imperfect and perfect while in bisyllabic verbs the vowel remains constant. The pronominal elements

<sup>11</sup> Moreno (1940), 298. For the morphemes in question see Moreno (1938), 41 (present tense: 3rd m. *-es*, 3rd f. *-aus*); 43 (subordinate perfect: 3rd m. *-idi-* f. *-ada*); 46 (principal perfect: 3rd m. *-id-es*, f. *-ad-us*) as regards the main conjugation, and 50-55 for the other conjugations (negative, interrogative positive and negative). See also the discussion by Cerulli (1938b), 213ff.; 13f. for Janjero; (1951), 119 for Kaffa,

distinguish gender in the second and third persons singular and the second person is further characterized by the suffixes *-a* m., *-i* f., to give the following pattern in the singular: 1st c. *a-*, 2nd m. *tí—a*, 2nd f. *tí—i*, 3rd m. *i-*, 3rd f. *tí-*. The agreement with Eastern Semitic (Akkadian) is evident from a comparison of the paradigms of the perfect:

	Bedawye	Akkadian
sg.	1st c. <i>á-ktib</i>	<i>a-prus</i>
	2nd m. <i>tí-ktib-a</i>	<i>ta-prus</i>
	2nd f. <i>tí-ktib-i</i>	<i>ta-prus-i</i>
	3rd m. <i>i-ktib</i>	<i>i-prus</i>
	3rd f. <i>tí-ktib</i>	<i>ta-prus</i>
pl.	1st c. <i>ní-ktib</i>	<i>ní-prus</i>
	2nd c. <i>te-ktib-na</i>	<i>ta-prus-ā</i>
	3rd m. <i>e-ktib-na</i>	<i>i-prus-ū</i>
	3rd f.	<i>i-prus-ā</i>

For monosyllabic verbs: 1st c. *e-dār*, 2nd m. *te-dār-a*, 2nd f. *te-dār-i*, 3rd m. *e-dār*, 3rd f. *te-dār* (from *dīr* 'to kill'); present: *a-n-dār*, *té-n-dīr-a*, etc. Note the *-n-* infix, and the vowel *e* in the prefix.

## II

In the hope that the above presentation of the Cushitic linguistic material connected with the expression of gender has not been too insufficient or confused, we will now briefly review the main results in order to allow a general critical appraisal.

One feature that all the languages of the Cushitic group have in common—shared, of course, with many other languages—is NATURAL (or SEX) GENDER. The difference between male and female is everywhere acknowledged. It is one of the commonest human experiences and an essential 'datum' of society. Therefore it is not surprising that languages should have distinct lexemes (heteronymy) for the naming of male and female human beings (man, woman, boy, girl, etc.), and especially for family members (father, mother, son, daughter, etc.) as well as animals. It may strike us as less obvious that, although everywhere present, natural gender is not always expressed through heteronymy. Not infrequently gender seems to be overlooked, the same lexeme or word being used indiscriminately for male and female. We have already quoted the examples of Bedawye *'or*, Somali *walāl*, Sidamo *rodó*, all meaning 'brother' or 'sister', Ometo *bēto* 'son' or 'daughter', etc. (cf. English *child*, German *Kind*, Italian *infante*, etc.). The ambiguity that may arise in such cases can be overcome by adding to the epicene term a qualifier meaning 'male' or 'female', a procedure known to many languages, especially those lacking grammatical gender (cf. English *he-goat*, *jackass*,

etc.). An extreme case is offered by Janjero where the term *asu* can mean both 'man' and 'woman'. The speaker therefore normally says *adk asu* (literally 'male man') for 'man' and *mašk asu* (literally 'female man') for 'woman' (Cerulli 1938b, 52, § 41). This is paralleled in Sumerian, the first written language known to us, in which *dumu-mi* (literally 'son woman') means 'daughter'.

A further indication of the presence of gender is the fact that speakers may have the consciousness of gender in things or inanimate beings in general although the lexemes in question do not carry any gender marker. In such cases the concept of gender is revealed through concord in the article, in the denominatives, adjectives or qualifiers, in the pronouns or finally in the verb.

In contrast with what we call grammatical gender, this could be termed psychological gender. Such a situation is illustrated by Bedawye where, outside a few examples of heteronymy and the one exception of *tak-at* already mentioned, nouns have no markers for gender (Reinisch, 1893-1894, 59: "... äusserlich in der Form des Nomens selbst [ist] das Geschlecht durch kein spezielles Merkmal gekennzeichnet"). But in this case gender may manifest itself through the article which accompanies a noun or in its declensional endings or the predicate, as already mentioned. (For Tu-Bedawye, see Roper 1928, § 42, 12 f.).

Agaw also distinguishes gender in inanimate things but nothing in the noun itself marks it. The same must be said of the Omo sub-group of languages, where even natural gender is barely represented and the expression of gender is left to the verb (third person singular). Inanimate things are, as a rule, considered to be masculine.

As regards the semantic, or conceptual, implications of psychological gender, we shall have to return to this question after having first considered the formal aspect of grammatical gender and having seen what kind of markers are used in Cushitic.

Traces, at least, of grammatical gender may be found in some form or other throughout practically the whole of Cushitic. If we consider first the noun-adjective, we can make the following preliminary observation: Cushitic has as a common feature an 'absolute' form for the noun-adjective which is the usual form for the accusative and vocative cases. As regards gender, it is a 'non-marked' form. Indeed, when either the subject or gender is to be stressed, this is done by means of a morphological marker (in Sidamo Burgi *-(gu)-ni* for the nominative, in Galla *-n(i)*, in Ometo *-i/-y*, etc.). It should thus be noted that it is the unmarked form which functions as the normal or basic one. The marking of gender therefore establishes an opposition between the unmarked form and the marked one and, as it happens, between the absolute or primary form and the subject form.

As to the markers of gender, these will affect the feminine as opposed to the masculine as the unmarked form. To simplify matters we can distinguish two main types of morpheme: A, a first type having as principal feature a vowel or a vowel plus consonant other than *t*; B, a second type in which (feminine) gender is marked



by means of morphemes characterized by the presence of a consonant which, in most cases, is *-t*.

Type A is found in its purest form in Saho-‘Afar. Here, nouns with vowel termination (*-ā, -ē, -ī, -ō, -ū*) are feminine, all others masculine. In Agaw type A occurs together with type B. As against the numerous suffixes that are added to the root of the noun (*-ā, -ē, -ī, -ō, -ū; -n* plus vowel; -vowel plus *n* plus vowel; *-n* plus consonant; etc.), two suffixes are specialized as markers of the feminine:

(1) *-ī*, not very frequent, in Kemant and in the two extreme dialects of Agaw (i.e. Bilin and Awiya), although the former has developed it more than the latter. This suffix properly belongs to the adjectives.

(2) *-ā*, more used in Damot than in Awiya. As already remarked, in Bilin adjectives form the feminine by means of the suffix *-ī*. Relative adjectives show an opposition m. *-āw*, f. *-ē*, pl. *-ō* (Quara); m. *-aw*, f. *-rəy*, pl. *-awk* (Khamir); m. *-awh*, f. *-rərī*, pl. *-aw* (Bilin). Denominative adjectives oppose: m. *-ū* to f. *-ē* (Quara), m. *-ū* to f. *-ī* (Khamir), m. *-uh* to f. *-rī/-dī* (Bilin).

Galla has a type A feminine for adjectives, opposing m. *-ā* to f. *-ō*; note also the plural suffix *-ō-t-a*. The opposition m. *-ā* f. *-ī/ē* is found in the personal pronoun in the third person singular of the object case.

More common is type B, with suffix *-t*. In Somali abstract feminine nouns are formed by means of the affixes *-āh, -in, -nin, -nīmo*, as against masculine abstract nouns which are in *-is*. In Arabic loanwords *-o/-a* occurs, representing the Arabic feminine termination *-at<sup>un</sup>*. Nomina agentis distinguish m. *-ōw*, f. *-lēy*.

Walamo and Zala have m. *-a*, f. *-o* in the demonstrative (*(h)anna/o*). Janjero and Kaffa have a feminine in *-ē*, corresponding to Sidamo *-ē* and Agaw *-ī*. Note that the vowel *-a*, although it does occur as a distinctive mark of the feminine, is nonetheless very rare. It is found more commonly, along with other vowels, as a masculine suffix for nouns.

Type B, characterized by the consonant *-t*, enjoys a wider diffusion.

In Bedawye, which marks gender in one noun only (*tak-at* ‘woman’), the *-t* is characteristic in: (a) the article; (b) the declension of the noun (*yās* ‘dog’, *tūyas* ‘the bitch’, *yas-t* ‘a bitch’) in the nominative and accusative indefinite and in the genitive. In the construct syntagma, should both rectum and regens be feminine, the feminine characteristic is repeated twice (*-t-i-t*); (c) the predicative form of the adjective (*nigis-u* m., *nigfs-t-u* f. ‘dirty’). The same terminations are also affixed to other parts of speech when used predicatively; (d) the personal pronoun and the demonstrative pronoun. It can be seen that the *-t* characteristic of the feminine is here in full evidence.

Agaw has rare occurrences of a *t* feminine marker. Of interest is the fact that *t* can also occur as a prefix: *t-eḡr-i* (Bilin), *t-ayr-i* (Quara) ‘aunt’, from *eḡer* ‘father’. Here



types A and B are used together. Perhaps we could consider the opposition m. *-uḥ* f. *-ri/-dī* of Bilin, quoted under type A, as etymologically connected with type B on account of the fact that in Agaw, as remarked above, *-t* can become *-r*.

Saho-ʿAfar distinguishes gender in individualis forms by means of a stress opposition: m. *-ta*, *-to* (*-y-ta/o* after a vowel), f. *-tá*, *-tǒ*. Although *t* is here not directly connected with gender, we shall see that it is not entirely irrelevant for our purpose.

As we have seen, in order to build the subject form of the noun, besides *-n(i)* Galla can add *-ti* and *-ti* is an old termination of the feminine. In feminine nouns it enjoys wider diffusion in Southern Galla even when the noun does not end in *-ā* (absolute form). As was observed for Agaw, types A and B may also co-occur in Galla analogically in the formation of the subject (*laf-t-ín* 'the earth'). Adjectives in *-ā* can form the feminine either by *-ā* becoming *-ō* or *-tu* (*dím-tu* 'red' f. from *dímā*); similarly, adjectives in *-ēsā/-eččā* have their feminine in *-etti*, those in *-aččā* in *-atti*, and those in *-iččā* in *-itti*.

Sidamo has the same feminine element *t* in the feminine suffix *-te* (cf. *-ti* in Galla). With vowel *e* it occurs in the plural form *-te*, which is the feminine article. When gender is marked in the adjective this is done by means of the suffix *-ta/e* in the primary form and *-te/i* in the subject form. Further, *-te/i* is the demonstrative feminine pronoun meaning 'this' and *-to/i* enters into the formation of the feminine pronoun meaning 'that'.

As regards the expression of gender in the PERSONAL PRONOUN, freestanding and suffixed, and in the verb we have the following data:

Bedawye marks gender by means of the opposition *r/t* (internal) and *k/s* (external). In the second person singular suffix pronoun masculine and feminine are distinguished by opposing *-(k)a* to *-ki* (i.e. by vowel opposition, as in Semitic *-ka*, *-ki* object and *-ta*, *-ti* subject). The same vowel opposition (*a/i*) also occurs in the verbal endings.

Low Cushitic marks the opposition of gender in several classes of pronoun by means of the opposition *k/t*, for masculine and feminine respectively (Saho-ʿAfar, Galla and Sidamo).

A suffix *-tē/i* forms the plural of the second person of the personal pronoun in four out of seven languages of the Omo region (Walamo, Zala, Gofa, Basketo): *in-tē/i*, to be compared with Janjero *ittō*, Kaffa *itto-ši* (from *\*in-to-*) and finally with Hamito-Semitic *\*at*, *\*ant* 'thou' (Cerulli 1938b, 211).

In the VERB we have seen how gender affects only the third person singular, where masculine and feminine are indicated by the same elements that serve to distinguish the first from the second person. The same applies in pattern A (*a/ta*). In pattern B the third person masculine is also differentiated from the second person masculine, by means of *ya* taking the place of *a* and in the feminine by *ti* or *t-i* taking that of *-ta*. As we have seen, the quality of the vowel changes according to the tense of the verbal form.

## III

*Semantic and conceptual implications of gender*

Natural gender has been shown to be present in all the languages of the Cushitic group. The differentiation of sex is undoubtedly a fundamental datum of man's experience and one of the elemental facts upon which society is based. It would therefore seem obvious to derive all concepts and manifestations of gender from the experience of sex distinction in nature. Man would thus have applied to the things of the world around him a reflection of the difference which exists between the sexes. He could have been prompted in this by the fact that, originally, man was inclined to attribute a soul, a life, to all things and, as a consequence, also that manifestation of life which is sexual differentiation. For Cushitic such a view was held by Reinisch, who wrote (1893-1894, 59, § 110): "die Unterscheidung der beiden Geschlechter ist ursprünglich gewiss vom Sexus ausgegangen ...". Grimm already held similar views regarding the Indo-European languages. But ethnology and cultural anthropology draw our attention to the fact that other fundamental human experiences exist alongside the basic consciousness of sex. Indeed, man lives in an environment which is sometimes harsh and felt by him to be hostile or sometimes, conversely, felt to be favourable and good towards him. Under the impact of surrounding reality man is led to view himself, along with his family and kinsmen, as opposed to or distinguished from what appears to him as 'otherness'. Human beings are thus set apart from all other beings. Such is the situation already mentioned with regard to Sumerian, which distinguishes two classes, of 'persons' on the one hand and of 'things' on the other.

This concept would explain the EPICENE terms covering both 'man' and 'woman', 'male' and 'female', since they are considered to belong to the same 'inclusive' class. When the interest centers on individuals of the same class, then natural gender will give rise to sex distinction (heteronymy). If, however, man's attention is drawn to the variety of things in the world confronting him, he may give expression to his evaluation of external reality by developing more or less complicated patterns, as in the 'classes' of the Bantu languages. The approach as outlined here, although rather simplified, is essentially the one followed by Meinhof (1912, 22 ff.), who uses the term 'psychologisch'; although this is in explanation of the origin of 'polarity' (1912, 19) the term is equally applicable here. Homburger (1938, 4 f.), speaking against such a psychological explanation, remarks that classes reflect rather the more frequent associations with morphemes determining 'quantity': one ox, some oxen, (some) water, butter, the weather, (hard) times, the soldier (generic), and so on.

Let us consider a few facts:

- (1) Nouns with no morphological mark of gender may be considered to be masculine or feminine, as can be seen from concord in the article, demonstratives, adjective or

verb. Since gender is not of itself an inherent property of things, it must have been attributed to them by the speakers. By what process?

(2) A first indication may be found in those languages in which the gender of words is not (like grammatical gender) fixed but can vary according to the wish or value judgment of the speaker. Such 'mobile' or 'changing' gender is found in Fulani (which is not, of course, a Hamitic language). The same situation exists in the Agaw sub-group, as has been noted by Reinisch and again by Conti Rossini (1912, 111 f., with examples quoted from Khamir, Quara, Awiya): "le même mot indiquant un être inanimé peut changer de genre ... peut-être cela représente les derniers restes d'un système de divisions des noms par classes de rang et de valeur, système analogue à celui qui existe encore en bantou". As for Sidamo we are told by Moreno (1940, 23) speaking of the list quoted by himself that "these feminine names, even those denoting animate beings, have often a masculine construction".

This last remark reminds us of the case of Bedawye, where an animal as female as the cow (*ša'*) is considered masculine in gender on account of the animal's importance in the economic and social structure of rural life. But the same word is considered feminine when it denotes 'meat' (Reinisch 1893-1894, 60). In Galla "for certain animals like *harrê* 'ass', *gángē* 'mule', *hanṭuta* 'mouse' there is a tendency to consider them as feminine when their sex is not in question" (Moreno 1939, 138).

(3) A third point to be considered is what semantic values are connected with the feminine apart from natural gender. In Bedawye, while the masculine also serves to express "Grösse, Ansehen und Energie", the feminine in most cases expresses the contrary: "Kleinheit, Schwäche, und Passivität" (Reinisch 1893-1894, 60). In Tu-Bedawye "the names of inanimate things that are normally masculine are regularly made feminine to indicate a diminutive" (Roper 1928, 11, § 34).

As we have seen, in Agaw lexemes expressing inanimate things can change their gender according to the idea the speaker wishes to stress: largeness, volume or smallness, weakness, etc. (cf. Conti Rossini 1912, 111 f.).<sup>12</sup>

In Galla the names of small animals are normally feminine, which carries a diminutive and pejorative value. Ignoring gender, it can assume the value of a neuter (Moreno 1939, 138). According to Meinhof (1912, 23) the situation would however be the other way about: "Das Umgekehrte wäre richtig. Das Femininum wird in der Regel durch das Neutrum ausgedrückt". This is in accord with his theory directed against the primacy of sex.

A wider range in the use of the feminine is found in Sidamo. Here the feminine can be used for names of small animals (cf. Galla) as well as for cereals, liquids and paired parts of the body: 'eyes', 'ears', 'hands', 'feet' as well as 'nose' (*sano*). Kaffa

<sup>12</sup> The same semantic phenomenon (diminutives treated as feminine) is present in many languages, such as Nilotic, Nilo-Hamitic, Berber, etc.; see Tucker and Bryan (1956), 154 and note 8.

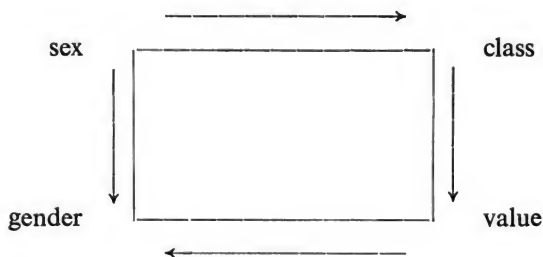
behaves in the same way as Galla. Within Hamitic we can also quote the case of Egyptian, which uses the feminine not only for natural gender but also for abstract nouns, for collectives, neutral expression such as 'what, that which', for various nouns denoting objects (e.g. *nš.t* 'throne', *w'r.t* 'leg') etc. and, finally, for names of foreign countries, cities and, at times, for nomes.<sup>13</sup>

(4) The connection of the feminine (morphological gender) with collectives and abstract nouns also explains the fact that the plural can sometimes be expressed by means of the morphemes of the feminine. Agaw, for example, forms the plural of nouns ending in *-ā* by shortening the noun, by dropping the *-ā* or changing it to *-i*. The explanation given by Conti Rossini (1912, 115, § 113) is that the *-ā*, which corresponds to Bilin *-ra*, makes of the noun a *nomen unitatis* and when it is dropped the noun regains its collective value. Bilin and 'Afar add the feminine ending *-t* (Sidamo *-te*). Now the collective, together with the abstract, set against the *nomen unitatis* and the concrete, may be considered to be a secondary category, that is to say in terms of gender as feminine. As Conti Rossini remarks (1912, 114, § 117), the same elements that serve to distinguish the feminine grammatically also serve to denote the collective or abstract plural, a fact well known also in Indo-European and Semitic languages. And this may also be the origin of 'polarity' as it occurs within Hamitic, for example in Somali where nouns can change gender by passing into the plural.

In an attempt to synthesize the several criteria upon which are based classifications for gender and number, we can set up the following table:

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
masculine	animate	persons	I/thou	individual	concrete	singular	major
feminine	inanimate	things	he/she	collective	abstract	plural	minor

This may be stated in a simpler and more comprehensive way by reducing it to its essential components arranged in the form of a square:



<sup>13</sup> See Erman (1902), 49f.; Gardiner (1957), especially §§ 51 and 92; for Hebrew see Joüon (1947), §§ 134-136; for Arabic see Wright (1967), 177-184. As pointed out at the Colloquium by F. W. Par-

(1) sex and class react on each other; (2) value is the point of confluence of the classes; (3) gender is the result of the confluence of sex, class and the subjective criterion of value.

The table lists the main mutually opposable categories which govern the classification of nouns with respect to gender and number. The analogy, even if it is only a broad one, with the classes of Bantu would appear undeniable.<sup>14</sup> Class (1), natural gender, is expressed through heteronymy applied to human beings and animals; class (2) is reflected in the epicene terms for 'male' and 'female', as opposed to common nouns; class (3) combines the expression of gender, either natural or grammatical, through concord or particular morphemes; class (4) has been discussed at some length in connection with the verb; classes (5) and (6) are revealed in the formation of nouns through affixation and/or structural change to give the *individualis* or *nomen unitatis* and the absolute or primary form of collectives (-čō: Sidamo *os-iččō* 'the child', from *oso*; Galla *motiččā* 'the king', from *môti*); special morphemes for employment with abstracts: *dimmū* 'cat', *dimmu-rā* 'a single cat', *dimāmu* 'cats', *dimmū-t* 'single cats'. Class (8) would explain the mobility of gender, as observed in Agaw and Bedawye in Cushitic, and in Fulani in Negro-African. The facts that it is possible for a noun to change its gender at the level of *parole* (in the Saussurean sense), according to the evaluation of the speaker and that gender is determined on the basis of number, volume, value, quantity, importance, prestige, show that in the mentality underlying the Cushitic languages an important role is played by subjective evaluative criteria. These evaluations can be designated by means of such terms as 'major', 'minor', 'superior', 'inferior', 'higher', 'lower', etc. In conclusion it can be said that gender in Cushitic, far from being a mere grammatical category, reflects a very complex reality which pervades language in an interplay of many different categories arranged in sets of oppositions. Gender appears as the surface mechanism through which these categories find a more or less adequate mode of expression. Furthermore, the tendency is here already at work for gender to end up as a purely grammatical classification of nouns, with set morphemes. In Semitic, and even more so in Indo-European, grammatical morphemes have obscured the variety of the original classes, resulting in a degradation from 'concept, idea' to 'morphological or grammatical phenomenon'.

In the case of Semitic, and this must be our concluding remark, simplification in the expression of gender has reached a final stage. As against the normal form of the noun which, with few exceptions, carries the connotation of masculine gender, feminine gender is indicated throughout by the one morpheme *\*-at/t*. That *\*-at* is to be considered as a true feminine marker is proved by the pan-Semitic occurrence of the morpheme and it is further confirmed beyond reasonable doubt by the wide

sons, the same phenomenon occurs in Hausa where rivers, for example, are feminine; Olderogge (1954, § 15, 20) mentions as being feminine and mostly ending in *-a* < *\*-at* the names of regions and cities.

<sup>14</sup> For traces of 'classes' in Semitic see the pertinent remarks of Diakonoff (1965), 57, n. 3.

diffusion of the *t* morpheme in High and Low Cushitic or, in another classification, in Northern (Bedawye), Central (Agaw) and Eastern (Saho-'Afar, Galla) Cushitic, with traces in other sub-groups. The same morpheme is further used as a mark of the plural, in the demonstrative pronouns, etc. The other type of morpheme for the feminine, the A type which consists essentially of a vowel or vowels or a vowel plus consonant other than *t*, is found in the Western Sidamo languages which are farther removed from Semitic than the other sub-groups of Cushitic. We can further add that *\*-at* is present in Egyptian and in the Chad languages, which are classified with Hamitic (Greenberg 1955, 49 f.; Diakonoff 1965, 57). Finally, as regards its position in the word, the *t* element occurs as a suffix, as a prefix (in Agaw), as both a suffix and a prefix (in Berber), as an infix (in the verb), and this fact that it can occupy all these different positions rules out the possibility of the *t* element being considered secondary (a *Hiatusilger*) to an original *-a*.<sup>15</sup>

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<sup>15</sup> It has recently been suggested that *-a*, and not *-t*, is the morpheme for the feminine in proto-Semitic (Akkadian) (Gelb, 1969, 21f.; 218f.).

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## DISCUSSION

**BENDER** This paper of Professor Castellino is extremely interesting. Now Harold Fleming at Boston University has been working with this problem of Cushitic classification and has found what he thinks are a number of isoglosses between East Cushitic and West Cushitic which are of such a nature as to indicate that perhaps East Cushitic and West Cushitic really are not co-ordinate members of the same family but should be set up as two separate sub-families under the Afroasiatic heading. And one of these isoglosses in fact is the nature of gender treatment in East Cushitic as against West Cushitic,—or, as he prefers to call them, Cushitic and Omotic. The fact is that gender is treated quite differently in Omotic. The reflexes *k* and *t* for example, which you find in Cushitic, occur rarely in Omotic, but instead you do find some vowel alternations of the type I think involving *o* for masculine and *a* for feminine, which I believe is also found in some of the Nilotic languages.

**ANDRZEJEWSKI:** May I add that the idea of splitting the Cushitic languages into two groups already occurred to Moreno when he divided the languages according to the type of their pronouns and also to Professor Tucker and Miss Bryan in their book *Linguistic Analyses*, and I think myself that the group actually is too large to be handled as a unit.

**MARGARET BRYAN:** I would like to make a small additional comment on Professor Castellino's analysis of the various functions of the *t* morpheme in the Cushitic languages. Some years ago I wrote an article entitled "The *t/k* languages—a new substratum" in which I attempted to link the Cushitic languages with other languages in which a singular *t* morpheme is paired up with a plural *k* morpheme. I went so far as to say that I thought that there was a substratum underlying all these other languages which also underlay Cushitic. I would like to take that back. But I would remind people that I did say then that there is this singulative function of *t* in the Cushitic languages and I would like to suggest that, though not belonging to a substratum, it might perhaps have entered Cushitic by the influence of the innumerable surrounding languages in which there is singular use of the morpheme *t* and in particular singulative rather than merely singular as opposed to plural; thus you do get these two elements *t* side by side in the Cushitic languages and, I believe, at some times having become fused. Now this might throw a little further light on the question



of so-called polarity. So you do get a feminine without regard to number, which is the pure pan-Hamito-Semitic feature, a particularizing or singulative form without regard to gender which has come in from other languages, and this I believe would account perhaps for the Saho *ita* which is without distinction of gender; there is, I know, a tonal distinction but the *t* element has nothing to do with gender. And then again in Somali where *t* is feminine and *t* is singular, *k* is the opposite of *t*: therefore *k* is masculine when it is being the opposite of feminine, plural when it is being the opposite of singular, and so you get this criss-cross of polarity.

PETRÁČEK: One can naturally question certain individual points in this very useful paper. For instance, the statement that "Bilin is the most archaic and pure language of the group". I think that Palmer has shown clearly that Bilin is in no way special from the point of view of phonology. But what is important is the methodology. I am happy to see here how our colleague has examined for a purely grammatical category these relationships of very different order. It is I think a dynamic approach and to be welcomed.

CASTELLINO: I am aware that my paper is no more than a tentative exercise in a field in which I am not really at home. I took up the subject in order to see whether it was possible, through the intermediary of Cushitic, to say anything definite about the morphology of gender in Semitic. My last remark was really in agreement with what Diakonoff said in his letter about the limitations of Gelb's theory of a feminine gender expressed simply by *a*. And as the ordinary form of the feminine in all Semitic languages is expressed directly, or indirectly when it has fallen, by *t*, and if one sees that *t* is also pre-eminent in a group of languages of the Hamito-Semitic family then one could say perhaps that it is not a *Hiatusstilger* when it occurs in Semitic but it must perhaps be one of the two elements that go to make up the morphology of gender. And then, upon looking into the Cushitic languages I became aware that the question was more complex than I had at first thought, that other categories of thought, we could say of grammar, come in to determine not only the morphology but also the use of the *t* element as it appears in the last part of my paper. And then I noticed that in Semitic languages also you have several categories conflated in this *t* morpheme.



VERBS WITH VOCALIC MUTATION IN SOMALI  
AND THEIR SIGNIFICANCE FOR  
HAMITO-SEMITIC COMPARATIVE STUDIES

B. W. ANDRZEJEWSKI

There are four verbs in Somali which have vocalic mutation<sup>1</sup> in their roots, i.e. have different vowels in different grammatical forms while retaining the same or a closely resembling consonantal framework; e.g.

<i>wùu y-idaah-daa</i> <sup>2</sup>	'he says'
<i>wùu y-idi</i>	'he said'
<i>wùu y-imaad-daa</i>	'he comes'
<i>wùu y-imid</i>	'he came'
<i>wùu y-qal</i>	'he stays, he remains'
<i>wùu y-iil</i>	'he stayed, he remained'
<i>wùu y-qaan</i>	'he knows'
<i>wùu y-iqiin</i>	'he knew'

The four verbs in question are: *yidi* 'to say', *yimi* 'to come', *yil* 'to stay, to remain' and *yiqiin* 'to know'<sup>3</sup> and I propose to refer to them collectively as 'mutating verbs'. They are unique in the Somali language, since no other verbs have vocalic mutation in their roots.

<sup>1</sup> I use this term in the sense of 'apophony in which the distribution of variants is NOT determined by the phonological environment'. In this respect mutation, as understood here, contrasts with the concept of automatic alternance, where the selection of variants is determined by the phonological environment.

<sup>2</sup> The transcription of Somali used in this paper is explained in Andrzejewski (1964) and Galaal (1956) except that the symbol *q* has been replaced by *ḡ*. The examples are taken from Northern Somali but it should be noted that the formulations given here also apply, though with some modifications, to all the other dialects of Somali except those of the Central Dialect Type (mainly spoken in the Upper Juba Province).

<sup>3</sup> When whole verbs (i.e. all the forms of a verb, taken collectively) are referred to, the 3rd person singular masculine of the past general extensive is used as the representative form (i.e. lexical entry form) and is translated by the infinitive in English and not by the 3rd person singular of the past tense. I restrict this practice to the mutating verbs only.

In addition to vocalic mutation the four verbs under discussion have prefixes *y-*, *t-*, *n-* and *Ø-* (zero), as can be seen from the following two paradigms of the verb *yīḍi*:

Past general extensive<sup>4</sup>

1st sg.	<i>wàan</i>	<i>īḍi</i>	'I said'
2nd sg.	<i>wàad</i>	<i>t-īḍi</i>	'you (sg.) said'
3rd sg. m.	<i>wùu</i>	<i>y-īḍi</i>	'he said'
3rd sg. f.	<i>wày</i>	<i>t-īḍi</i>	'she said'
1st pl.	<i>wàannu</i>	<i>n-īḍi</i>	'we said'
2nd pl.	<i>wàydin</i>	<i>t-īḍaah-deen</i>	'you (pl.) said'
3rd pl.	<i>wày</i>	<i>y-īḍaah-deen</i>	'they said'

Past general restrictive

1st sg.	<i>anígàa<sup>5</sup></i>	<i>īḍi</i>	The same meanings as above, except that there is a special emphasis on the pronoun, e.g. 'I said (= It was I who said so)'.
2nd sg.	<i>adígàa</i>	<i>y-īḍi</i>	
3rd sg. m.	<i>iságàa</i>	<i>y-īḍi</i>	
3rd sg. f.	<i>iyádàa</i>	<i>t-īḍi</i>	
1st pl.	<i>annágàa</i>	<i>n-īḍi</i>	
2nd pl.	<i>idínkàa</i>	<i>y-īḍi</i>	
3rd pl.	<i>iyágàa</i>	<i>y-īḍi</i>	

In Somali only one other verb has prefixes in its conjugation, namely the verb *yqhay* 'to be'.<sup>6</sup> It should be noted, however, that the verb *yqhay* has no vocalic mutation in its root. The differences which occur in the vowel of its root are merely automatic alternances determined by the vowel of the immediately following suffix, e.g.

<i>bóqor bāan qh-ay</i>	'I am a chieftain'
<i>bóqor má ḡh-id</i>	'you (sg.) are not a chieftain'

This type of alternance can be explained in terms of the general phonological rules of the language, as can be seen from the following examples:

<sup>4</sup> The names of verbal paradigms used here are the same as in Andrzejewski (1968). One of the most common contexts of the extensive paradigms is: the indicator *waa* + a preverbal subject pronoun ... + verb. On the other hand, one of the most common contexts of the restrictive paradigms is: noun (or its syntactic equivalent) + the indicator *bāa* ... + verb which stands in concord with that noun. For the functions of *bāa* see Hetzron (1965).

<sup>5</sup> I.e. *anīga* + the indicator *bāa*, *adīga* + the indicator *bāa* etc. The first components here are 'substantive pronouns' (see Andrzejewski 1961), and function as syntactic equivalents of nouns in the examples here.

<sup>6</sup> This verb is quoted here in the 3rd person singular masculine of the present tense as its representative form. For detailed description of this verb see Andrzejewski (1969).

<i>füre</i>	'a key'
<i>furá-hán</i>	'this key'
<i>furi-hii</i>	'the key'

In each mutating verb<sup>7</sup> the root has several shapes which will be referred to as its grades. In the table below grades of the same root are arranged into columns, but it should be noted that within some root grades there are automatic alternances with two variants each. The selection of these variants is determined by the phonological environment: the first variant in each pair occurs when it is not followed immediately by a vowel and the second when it is immediately followed by a vowel. The variants within the same alternance are placed side by side, with a colon between them. The letter *V* represents a short vowel the quality of which is the same as that of the immediately following vowel.

Verb	<i>yîḍi</i>	<i>yîmi</i>	<i>yîil</i>	<i>yîqiin</i>
Root grades	<i>îḍaah</i>	<i>imaad</i>	<i>aal : aall</i>	<i>aqaan : aqaann</i>
	<i>îḍaa</i>	<i>imid</i>	<i>iil : iill</i>	<i>iqiin : iqiinn</i>
	<i>îḍi</i>	<i>imi</i>	<i>ool : ooll</i>	<i>oq</i>
	<i>ḍe : ḍVh</i>	<i>im</i>		
	<i>oḍ</i>			

An overall view of the distribution of these root grades can be obtained from the selected tense paradigms given below as examples. A detailed account of the distribution of root grades and the selection of variants in alternances is given in the Appendix, since it is of relevance only to those who are interested in particular in the Cushitic group of languages.

<sup>7</sup> Some forms of mutating verbs have the quality of 'frontness' in their vowels, marked here with a cedilla under the first vowel letter of the form; for a definition of this feature which contrasts with 'backness' see Andrzejewski (1964) and Galaal (1956). The occurrence of frontness and backness in the mutating verbs follows the same rules as those which apply to weak verbs, the 2nd person singular imperative of which has 'backness' (i.e. whose vowels belong to the back series) as stated in Andrzejewski, 1968, 22, except that in addition, the following forms have frontness

(a) all forms of mutating verbs which occur with the prefix *y* or the ending *iin*,

(b) in the case of the verb *yîil* only: all forms of the past general and present-past general, all forms of the continuous tenses, all forms of the infinitive and infinitive dependent, negative optative and those forms of the negative conditional and negative conditional dependent which have the root grade *ooll*,

(c) in the case of the verb *yîqiin* only: all the forms which have the root grade *iqiin*: *iqiinn* and all forms of the past general.

## Imperative

<i>qé</i>	'say (2nd sg.)'
<i>qáh-a</i>	'say (2nd pl.)'

## Present general extensive

<i>wàan</i>	$\left\{ \begin{array}{l} i\dot{d}aah-daa (i\dot{d}aa)(i\dot{d}aah-aa) \\ imaad-daa \\ aal (aall-aa) \\ aqaan (aqaann-aa) \end{array} \right.$	$\left\{ \begin{array}{l} \text{'I say'} \\ \text{'come'} \\ \text{'stay, remain'} \\ \text{'know'} \end{array} \right.$
<i>wàad</i>	$\left\{ \begin{array}{l} t-i\dot{d}aah-daa (t-i\dot{d}aa)(t-i\dot{d}aah-aa) \\ t-imaad-daa \\ t-aal (t-aall-aa) \\ t-aqaan (t-aqaann-aa) \end{array} \right.$	'you (sg.) say, come, stay, etc.'
<i>wùu</i>	$\left\{ \begin{array}{l} y-i\dot{d}aah-daa (y-i\dot{d}aa) (y-i\dot{d}aah-aa) \\ y-imaad-daa \\ y-qal (y-qall-aa) \\ y-aqaan (y-aqaann-aa) \end{array} \right.$	'he says, comes, etc.'
<i>wày</i>	$\left\{ \begin{array}{l} t-i\dot{d}aah-daa (t-i\dot{d}aa) (t-i\dot{d}aah-aa) \\ t-imaad-daa \\ t-aal (t-aall-aa) \\ t-aqaan (t-aqaan-aa) \end{array} \right.$	'she says, comes, etc.'
<i>wàannu</i>	$\left\{ \begin{array}{l} n-i\dot{d}aah-naa (n-i\dot{d}aa) \\ n-imaad-naa \\ n-aal (n-aall-aa) \\ n-aqaan (n-aqaann-aa) \end{array} \right.$	'we say, come, etc.'
<i>wàydin</i>	$\left\{ \begin{array}{l} t-i\dot{d}aah-daan (t-i\dot{d}aah-aan) \\ t-imaad-daan \\ t-qall-iin (t-aall-aan) \\ t-aqaann-iin \end{array} \right.$	'you (pl.) say, come, etc.'
<i>wày</i>	$\left\{ \begin{array}{l} y-i\dot{d}aah-daan (y-i\dot{d}aah-aan) \\ y-imaad-daan \\ y-qall-iin (y-qall-aan) \\ y-aqaann-iin \end{array} \right.$	'they say, come, etc.'

## Past general extensive

<i>wàan</i>	$\left\{ \begin{array}{l} i\dot{d}i \\ imid (imi) \\ i\ddot{il} (i\ddot{ill}-ay) (qall-ay) (qollay) \\ i\dot{q}i\ddot{in} (i\dot{q}i\ddot{innay}) (aqaann-ay) \end{array} \right.$	'I said, came, etc.'
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<i>wàad</i>	$\left\{ \begin{array}{l} t-idi \\ t-imid \text{ (} t-imì \text{)} \\ t-ìil \text{ (} t-ìill-ay \text{)} \text{ (} t-qall-ay \text{)} \\ t-ìqiin \text{ (} t-ìqiinn-ay \text{)} \text{ (} t-qaann-ay \text{)} \end{array} \right.$	'you (sg.) said, came, etc.'
<i>wùu</i>	$\left\{ \begin{array}{l} y-ìdi \\ y-imid \text{ (} y-ìmi \text{)} \\ y-ìil \text{ (} y-ìill-ay \text{)} \text{ (} y-qall-ay \text{)} \text{ (} qoll-ay \text{)} \\ y-ìqiin \text{ (} y-ìqiinn-ay \text{)} \text{ (} y-qaann-ay \text{)} \end{array} \right.$	'he said, came, etc.'
<i>wày</i>	$\left\{ \begin{array}{l} t-idi \\ t-imid \text{ (} t-imì \text{)} \\ t-ìil \text{ (} t-ìill-ay \text{)} \text{ (} t-qall-ay \text{)} \\ t-ìqiin \text{ (} t-ìqiinn-ay \text{)} \text{ (} t-qaann-ay \text{)} \end{array} \right.$	'she said, came, etc.'
<i>wàannu</i>	$\left\{ \begin{array}{l} n-ìdi \\ n-imid \text{ (} n-imì \text{)} \\ n-ìil \text{ (} n-ìill-ay \text{)} \text{ (} n-qall-ay \text{)} \\ n-ìqiin \text{ (} n-ìqiinn-ay \text{)} \text{ (} n-qaann-ay \text{)} \end{array} \right.$	'we said, came, etc.'
<i>wàydin</i>	$\left\{ \begin{array}{l} t-ìdaah-deen \text{ (} t-ìdaah-een \text{)} \\ t-imaad-deen \\ t-ìill-een \text{ (} t-qall-een \text{)} \\ t-ìqiinn-een \text{ (} t-qaann-een \text{)} \end{array} \right.$	'you (pl.) said, came, etc.'
<i>wày</i>	$\left\{ \begin{array}{l} y-ìdaah-deen \text{ (} y-ìdaah-een \text{)} \\ y-imaad-deen \\ y-ìill-een \text{ (} y-qall-een \text{)} \text{ (} qoll-een \text{)} \\ y-ìqiinn-een \text{ (} y-qaann-een \text{)} \end{array} \right.$	'they said, came, etc.'

## Negative past general

<i>má</i>	$\left\{ \begin{array}{l} oq-án \text{ (} oq-annín \text{)} \text{ 'I, you, etc. (all persons) did not say'} \\ im-án \text{ (} im-annín \text{)} \text{ 'I, you, etc. (all persons) did not come'} \\ qoll-ín \text{ (} qol \text{)} \text{ 'I, you, etc. (all persons) did not stay, remain'} \\ oq-qon \text{ (} oq-oonnín \text{)} \text{ 'I, you, etc. (all persons) did not know'} \end{array} \right.$
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Although the verbs with vocalic mutation are only four in number, they occupy an important position in the Somali verbal system since they occur very frequently in all styles of speech. Moreover they present a puzzling problem from the comparative point of view. Vocalic mutation as a morphological device within the verbal system is rare in Cushitic languages. As far as I have been able to ascertain, apart from Somali, only Beja, Afar and Saho use it.

In Beja<sup>8</sup> it functions in a variety of ways as can be seen from the following examples:

*ginaf* 'to kneel down'  
*min* 'to shave'  
*bir* 'to snatch'  
*aktīm* 'I arrived'

*ginif* 'to cause to kneel'  
*atomān* 'to be shaved'  
*bār* 'snatching (nomen actionis)'  
*akantīm* 'I arrive'

In Afar<sup>9</sup> and Saho<sup>10</sup> vocalic mutation appears to occur mainly in different paradigms of the same verb:

Afar

*eme'te* 'I came'

*ama'te* 'I come, I will come'

Saho

*i-lika* 1st sg. aoristic perfect } of *lāka* 'to send'  
*ā-lāko* 1st sg. subjunctive }

Vocalic mutation in Somali, Beja, Afar and Saho has its obvious parallels in Semitic languages, even though there are considerable differences of detail. It is important to observe in this connection that vocalic mutation in Somali, Beja, Afar and Saho is restricted to those verbs which have the prefixes *y-*, *t-*, *n-*, *Ø-*,<sup>11</sup> again a feature with obvious parallels in Semitic languages. It seems most unlikely that two such morphological devices as vocalic mutation and prefixes similar in shape and distribution could be due to accidental resemblance.

Yet we do not know enough about the nature of the relationship between the Cushitic and Semitic languages<sup>12</sup> to put forward any firm hypothesis. If there ever existed a common Proto-Hamito-Semitic family of languages, did it have both

<sup>8</sup> Examples for Beja are from Roper (1929). (In the first five items reading across only stems are given).

<sup>9</sup> Examples for Afar are from Mahaffy (n.d.).

<sup>10</sup> Examples for Saho are from Reinisch (1878).

<sup>11</sup> In Cushitic languages, apart from Beja, Afar, Saho and Somali verbal prefixes occur only in the Agaw group, and even then in a very limited number of verbs; see Hetzron (1969), 44-45.

<sup>12</sup> The main difficulty in Hamito-Semitic comparative studies arises from the fact that so far no one has discovered regular sound correspondences which could account for the relationship between the branches of this group or even between the individual languages. The technique of mass comparison proposed by Joseph H. Greenberg, which is considered by some scholars as a substitute for the established methods of comparative linguistics, has failed so far to provide convincing results. The list of assumed Afro-Asiatic cognates in Greenberg (1963) is very small and it is difficult to enlarge it upon investigation into the lexical material of the languages involved, unless one is extremely generous in the assessment of what constitutes similarity. The movement towards trying to establish regular sound correspondences, as in the Indo-European or Bantu field, is still not extensive enough in our field, though some welcome developments must be noted, in particular Diakonoff's work on Semito-Hamitic comparisons. Another welcome step, though on a smaller scale, is Harold C. Fleming's endeavour to discover regular correspondences in the Cushitic group, in Fleming (1964).



vocalic mutation and prefixes in its verbal system? If so why did the Cushitic languages either abandon or reduce the use of these morphological devices?<sup>13</sup> Could it be due to the influence of some substratum or adstratum to which vocalic mutation in verbal forms was as alien as the prefix conjugation?

#### APPENDIX

##### *Distribution of root grades in mutating verbs in Somali*

The distribution of root grades of mutating verbs in Somali is described here by reference to the structure of weak verbs as formulated in Andrzejewski (1968). This can be done, since apart from prefixes, mutating verbs are analyzable into the same components as weak verbs: root, root extension, intermedium, link and ending.

##### *Prefixes*

Prefixes of mutating verbs are arranged here into three groups which will be referred to as Prefix Distribution Patterns, abbreviated to PDP.

	PDP 1	PDP 2	PDP 3
1st sg.	Ø-	Ø-	} Ø-
2nd sg.	t-	y-	
3rd sg. m.	y-	y-	
3rd sg. f.	t-	t-	
1st pl.	n-	n-	
2nd pl.	t-	y-	
3rd pl.	y-	y-	

##### *Roots*

The distribution of root grades is given in separate statements for each of the four verbs (Tables I-IV).

##### *Root extensions*

Root extensions of mutating verbs are the same as in weak verbs and are referred to by the same code letters as in Andrzejewski (1968). When the root extension is not stated in Tables I-IV it is then implied that it is Ø (zero). It should be noted that the root extensions in mutating verbs, unlike those of weak verbs, do not have any specific functions as modifiers of the meanings of the root.

<sup>13</sup> It is an interesting feature of the prefix conjugation in Cushitic languages that prefixes occur both in the perfective and imperfective tenses, thus showing a levelling tendency which reduces the contrastive function of this morphological device. This could have been the first step towards abandoning it altogether over a long period of time in other Cushitic languages.

*Intermedia*

Like roots, the intermedia in mutating verbs are the same as in weak verbs and have the same semantic function (expressing the continuous nature of a particular action or state).

*Links*

The consonantal links which occur between the ending and the preceding component of the verbal form in mutating verbs are different both in their shapes and distribution from those of weak verbs. They have three shapes only: *d*, *n*, and  $\emptyset$  (zero). The Link Distribution Patterns (abbreviated to LDP) are given below:

	LDP I	LDP II	LDP III
1st sg.	$d : d(\emptyset)$	$\emptyset$	} $\emptyset$
2nd sg.	$d : d(\emptyset)$	$\emptyset$	
3rd sg. m.	$d : d(\emptyset)$	$\emptyset$	
3rd sg. f.	$d : d(\emptyset)$	$\emptyset$	
1st pl.	<i>n</i>	$\emptyset$	
2nd pl.	$d : d(\emptyset)$	$d : d(\emptyset)$	
3rd pl.	$d : d(\emptyset)$	$d : d(\emptyset)$	

In the automatic alternance  $d : d(\emptyset)$  the variant *d* occurs when the root ends in *d* and  $d(\emptyset)$  (i.e. *d* with an optional variant  $\emptyset$ ) occurs when the root ends in *h*.

*Endings*

The only ending in mutating verbs which differs in shape from those of weak verbs is *iin*. There are, however, several differences in the distribution of endings as will be shown in Tables I-IV.

Some root grades occur exclusively with certain endings and to facilitate description all endings are arranged into four sets (abbreviated to S), according to their persons:

S 1	S 2	S 3	S 4
1st sg.	2nd pl.	1st sg.	1st sg.
2nd sg.	3rd pl.	3rd sg. m.	2nd sg.
3rd sg. m.		3rd pl.	3rd sg. m.
3rd sg. f.			2nd pl.
1st pl.			3rd pl.

In Tables I-IV, information as to what root grade occurs with what ending is provided as follows:

- (a) When no particular set is specified in the column in which endings are listed, it is to be understood that the root grade occurs with ALL the endings of the tense paradigm.
- (b) When a particular set is specified it means that the root grade occurs ONLY with the endings of that set within the tense paradigm.

### *Distribution Tables*

In the five tables below a column is given to each component of the verb, always in the order: prefix - root - root extension - intermedium - link - ending. When any of these components is left out in a particular table it is to be understood that that component is  $\emptyset$  (zero). Intermedium, link and ending, taken together, are referred to as terminations.

The following abbreviations are used: Rt. gr. (root grade), End. (ending), Ter. (termination). The letter W indicates that the particular ending or termination is identical with the corresponding ending or termination in weak verbs.

The phrase DO NOT OCCUR is placed in the column given to endings or terminations, when no examples of particular forms or tense paradigms have been found.

When PDP is not specified in the Tables it means that it is  $\emptyset$ . Similarly, when LDP is not specified it is to be interpreted as  $\emptyset$ .

All optional forms are given in brackets.

TABLE I

*Distribution of the root grades of the verb yiḏi*

Tense paradigm	PDP	Rt. gr.	LDP	End	
Imper.	3	<i>ḏe:ḏVh</i>	III	W	
Pres. gen. ext.	1	<i>iḏaah</i>	I	W	
		<i>(iḏaa)</i>	III	S1 Ø	
Pres. gen. res.	2	<i>iḏaah</i>	I	W	
		<i>(iḏaa)</i>	III	Ø	
Past gen. ext.	1	<i>iḏi</i>	II	S1 Ø	
		<i>iḏaah</i>		S2 W	
Past gen. res.	2	<i>iḏi</i>	III	Ø	
Past indep.	1	<i>iḏi</i>	II	S1 Ø	
		<i>iḏaah</i>		S2 W	
Optat.	1	<i>iḏaah</i>	I	W	
Rhet.	1	<i>iḏaah</i>	I	W	
Poten.	1	<i>iḏaah</i>	I	W	
Neg. pres. gen.	1	<i>iḏaah</i>	I	W	
Neg. cond.	1	<i>iḏaah</i>	I	W	
Pres. gen. dvg. A	1	<i>iḏaah</i>	I	W	
Pres. gen. dvg. B	1	<i>iḏaah</i>	I	W	
Past gen. dvg. A	1	<i>iḏi</i>	II	S1 Ø	
		<i>iḏaah</i>		S2 W	
Past gen. dvg. B	1	<i>iḏi</i>	II	S1 Ø	
		<i>iḏaah</i>		S2 W	
Pres. gen. cvg. A	2	<i>iḏaah</i>	I	W	
		<i>(iḏaa)</i>	III	Ø	
Pres. gen. cvg. B	2	<i>iḏaah</i>	I	W	
		<i>(iḏaa)</i>	III	Ø	
Past gen. cvg. A	2	<i>iḏi</i>	III	Ø	
Past gen. cvg. B	2	<i>iḏi</i>	III	Ø	
Neg. cond. dep.	1	<i>iḏaah</i>	I	W	
Tense paradigm			Rt. gr.	Ex.	Ter.
All cnt. tense paradigms, inf., neg. imper., neg. pres.-past gen., neg. opt., inf. dep., neg. pres.-past gen. dep. A, neg. pres.-past dep. B			<i>oḏ</i>	AN	W

TABLE II

*Distribution of the root grades of the verb yimi*

Tense paradigm	PDP	Rt. gr.	LDP	End
Pres. gen. ext.	1	<i>imaad</i>	I	W
Pres. gen. res.	2	<i>imaad</i>	I	W
Past gen. ext.	1	<i>imid (imi)</i>	II	S1 Ø
		<i>imaad</i>		S2 W
Past gen. res.	2	<i>imid (imi)</i>	III	Ø
Past indep.	1	<i>imid (imi)</i>	II	S1 Ø
		<i>imaad</i>		S2 W
Optat.	1	<i>imaad</i>	I	W
Rhet.	1	<i>imaad</i>	I	W
Poten.	1	<i>imaad</i>	I	W
Neg. pres. gen.	1	<i>imaad</i>	I	W
Neg. cond.	1	<i>imaad</i>	I	W
Pres. gen. dvg. A	1	<i>imaad</i>	I	W
Pres. gen. dvg. B	1	<i>imaad</i>	I	W
Past gen. dvg. A	1	<i>imid (imi)</i>	II	S1 Ø
		<i>imaad</i>		S2 W
Past gen. dvg. B	1	<i>imid (imi)</i>	II	S1 Ø
		<i>imaad</i>		S2 W
Pres. gen. cvg. A	2	<i>imaad</i>	I	W
Pres. gen. cvg. B	2	<i>imaad</i>	I	W
Past gen. cvg. A	2	<i>imid (imi)</i>	III	Ø
Past gen. cvg. B	2	<i>imid (imi)</i>	III	Ø
Neg. cond. dep.	1	<i>imaad</i>	I	W
Tense paradigm			Rt. gr.	Ex. Ter.
All cnt. tense paradigms, inf., neg. imper., neg. pres.-past gen., neg. opt., inf. dep., neg. pres.-past gen. dep. A, neg. pres.-past gen. dep. B			<i>im</i>	AN W
Imper.			DO NOT OCCUR	

TABLE III

*Distribution of the root grades of the verb yiil*

Tense paradigm	PDP	Rt. gr.	End
Pres. gen. ext.	1	<i>aal:aall</i>	S1 Ø (W), S2 <i>iin</i> (W)
Pres. gen. res.	2	<i>aal:aall</i>	Ø (W)
Past gen. ext.	1	<i>iil:iill (aall)</i>	S1 Ø (W), S2 W
	3	<i>(ooll)</i>	S3 W
Past gen. res.	2	<i>iil:iill (aall)</i>	Ø (W)
	3	<i>(ooll)</i>	S4 W
Optat.	1	<i>aall</i>	W
Poten.	1	<i>aall</i>	W
Neg. pres. gen.	1	<i>aal:aall</i>	S1 Ø (W), S2 <i>iin</i> (W)
Neg. cond.	1	<i>aall</i>	W
	3	<i>(ooll)</i>	S3 W
Pres. gen. dvg. A	1	<i>aal:aall</i>	S1 Ø (W), S2 <i>iin</i> (W)
Pres. gen. dvg. B	1	<i>aal:aall</i>	S1 <i>i</i> (W) (Ø), S2 <i>iin</i> (W)
Past gen. dvg. A	1	<i>iil:iill (aall)</i>	S1 Ø (W), S2 W
	3	<i>(ooll)</i>	S3 W
Past gen. dvg. B	1	<i>iil:iill (aall)</i>	S1 Ø (W), S2 W
	3	<i>(ooll)</i>	S3 W
Pres. gen. cvg. A	2	<i>aal:aall</i>	Ø (W)
Pres. gen. cvg. B	2	<i>aal:aall</i>	<i>i</i> (W) (Ø)
Past gen. cvg. A	2	<i>iil:iill (aall)</i>	Ø (W)
	3	<i>(ooll)</i>	S4 W
Past gen. cvg. B	2	<i>iil:iill (aall)</i>	Ø (W)
	3	<i>(ooll)</i>	S4 W
Neg. cond. dep.	1	<i>aal:aall</i>	W
	3	<i>(ooll)</i>	S3 W
Tense paradigm			Rt. gr. Ter.
Imper., pres. cnt. ext. (rare), pres. cnt. res. (rare), inf., neg. imper., neg. opt., inf. dep.			<i>ool:ooll</i>
neg. pres.-past gen., neg. pres.-past gen. dep. A			
neg. pres.-past gen. dep. B			
All cnt. tense paradigms except those shown above, past indep., rhet.			DO NOT OCCUR

TABLE IV

*Distribution of the root grades of the verb yiqiin*

Tense paradigm	PDP	Rt. gr.	End
Pres. gen. ext.	1	<i>aqaan:aqaann</i>	S1 Ø (W), S2 <i>iin</i>
Pres. gen. res.	2	<i>aqaan:aqaann</i>	Ø (W)
Past gen. ext.	1	<i>iqiin:iqiinn (aqaann)</i>	S1 Ø (W), S2 W
Past gen. res.	2	<i>iqiin:iqiinn (aqaann)</i>	Ø (W)
Optat.	1	<i>aqaann</i>	W
Neg. pres. gen.	1	<i>aqaan:aqaann</i>	S1 Ø (W), S2 <i>iin</i>
Pres. gen. dvg. A	1	<i>aqaan:aqaann</i>	S1 Ø (W), S2 <i>iin</i>
Pres. gen. dvg. B	1	<i>aqaan: aqaann</i>	S1 <i>i</i> (W), S2 <i>iin</i>
Past gen. dvg. A	1	<i>iqiin:iqiinn (aqaann)</i>	S1 Ø (W), S2 W
Past gen. dvg. B	1	<i>iqiin:iqiinn (aqaann)</i>	S1 Ø (W), S2 W
Pres. gen. cvg. A	2	<i>aqaan:aqaann</i>	Ø (W)
Pres. gen. cvg. B	2	<i>aqaan:aqaann</i>	<i>i</i> (W) (Ø)
Past gen. cvg. A	2	<i>iqiin:iqiinn (aqaann)</i>	Ø (W)
Past gen. cvg. B	2	<i>iqiin:iqiinn (aqaann)</i>	Ø (W)
Tense paradigm			Rt. gr.   Ex.   Ter.
Inf., neg. pres.-past gen., inf. dep., neg. pres.-past dep. A, neg. pres.-past dep. B, neg. opt.			<i>oq</i>   OON   W
Imper., all cnt. tense paradigms, past indep., rhet., poten., neg. imper., neg. cond., neg. cond. dep.			DO NOT OCCUR

TABLE V

*Selection of variants in alternances occurring within root grades of the verbs yidi, yiil and yiqiin*

Variant within alternance	Conditions of occurrence
<i>e</i>	When the root occurs with the ending Ø and is not immediately followed, without a pause intervening, by a word beginning with a vowel.
<i>Vh</i>	When the root occurs with an ending beginning with or consisting of a vowel, or when the root occurs with the ending Ø but is immediately followed, without a pause intervening, by a word beginning with a vowel.
<i>aal, iil, ool, aqaan, iqiin</i>	(a) Always: when the root occurs with the ending Ø. (b) As an optional alternative to <i>aall, iill, ooll, aqaann</i> and <i>iqiinn</i> respectively, when the root occurs with the ending <i>i</i> of the pres. gen. dvg. B or pres. gen. cvg. B.
<i>aall, iill, ooll, aqaann, iqiinn</i>	(a) Always: when the root occurs with an ending beginning with or consisting of a vowel other than the vowel of the ending <i>i</i> of the pres. gen. dvg. B or pres. gen. cvg. B. (b) As an optional alternative to <i>aal, iil, ool, aqaan</i> and <i>iqiin</i> respectively, under the same conditions as under (b) in the entry above.

*Note on accentual patterns:*

The accentual patterns of mutating verbs are the same as those of corresponding weak verbs except for the 3rd sg. m. past indep. of the verbs *yīḍi* and *yīmi*, which are *yīḍi* and *yīmi* respectively.

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## DISCUSSION

PETRÁČEK: The 'internal' type of plural is well represented in Bedauey and we find it in Somali also. In Berber there is a rich play of vowel alternation and a few examples even in Coptic. It may be chance but it seems to me that it is a Hamito-Semitic feature.

PARSONS: Plurals of nouns in Hausa do not have *ablaut* like those of Berber. It is however part of the verbal system, although it is not of course part of the verbal gradational system. But you get it in verbal nouns in a rather interesting way. It is generally with the derived long vowels, where you have a back and a front correspondence. For instance the word for 'to forge' is *kera* and the verbal noun is *kira*, and there are a great number like that. That does not by the way exclude *i* occurring in a verb *per se* as a phoneme (for instance you get *lek* 'to peep' and *lik* 'to stick', which are separate lexemes). The other one of course is *ō : ū*, which you get in *doka* 'to beat', *duka* 'beating'. There are a very few examples, but in basic words, where you get a simple alternation between the two vowel phonemes of Hausa, *a* and *i*. That is the only trace of this substratum, if substratum it be, in Hausa but I think there are many examples of it in other Chadic languages.

CARNOCHAN: As far as Bachama is concerned the question of vowel mutation in the stem of the verb is an extremely common feature, although not related to differences of tense but to differences of aspect and of singularity or plurality as far as the activity of the verb is concerned for verbs which have any vowel other than *a* as their stem vowel. For instance the verb *dimo* 'to sink': if you say 'the canoe sank' then you have the form *dimo*; if you say 'the canoes sank', with a plural form for 'canoes' then the form is *diemo*, and so on. If someone kills one man, the form will be *bəl*, if he kills two men—if there are two killings—it will be *bal*. This is also quite a common feature as far as the nouns are concerned.

HETZRON (by letter): In Southern Agaw also, one can detect traces of vocalic mutation in verbs. The personal suffixes of the definite aspect are quite obviously encliticized forms of the verb *yay-* 'to be' (cf. Hetzron 1969, 71-72). In the imperfect, this manifests itself in the form of a *γ* only, while perfect has *γ<sup>w</sup>*. This suggests that the past tense form of the verb 'to be' may have had an *u* as against the *a* of the present (today *a* is used in all tenses, cf. Hetzron 1969, 44-45). This assumed dichotomy *a/u* for imperfect/perfect is further confirmed by the subordinate forms *tambuta* 'as soon as he arrived' (with *u*) versus *tambáta* 'in order that he arrive' (with *á*) both containing the suffix *-ta* 'like'. If we accept the testimony of Amharic which must have copied its parallel forms (*əndā-dārrāsā/ənd-idārs*) from the Agaw substratum, *tambu-* is a perfect and *tambá-* an imperfect, *u/a* being traces of the auxiliary. A similar assumption can be made concerning *desáni* 'when I study' versus *desúni* 'if I study'

which, according to the testimony of Ethiopian Semitic, also reflect the dichotomy imperfect/perfect (Hetzron 1969, 74). The *e* of the imperfect versus the *a* in the perfect of the indefinite aspect suffixes, also originally auxiliaries, may also be mentioned here (Hetzron 1969, 72). Finally, there is one case in Southern Agaw where a comparable mutation is still operative, the so-called AB pattern (Hetzron 1969, 38, 2.O.3.A.3, with further references) which, for a limited number of verbs, requires mid tone in the perfect tenses and high tone in the imperfect tenses. Four out of the five verbs with prefix conjugation have this tonal mutation, and the rest of the verbs which display it also belong to the basic vocabulary ('to do', 'to find' and 'to save'). Similar tonal mutation may even be found in the suffixes *-ni/-ni* (temporal/conditional; Hetzron 1969, 74) and in the traces of the auxiliary *u/a* as mentioned above. Somali exhibits a clear prevalence of *a* in the imperfect as against *i* in the perfect (unless the stem is followed by a *d*). Southern Agaw has traces of a similar dichotomy *a/u*. I am not sure how to interpret the scarce data available to me on Bedaaye (infix *-an-* for the imperfect) and Saho (prefix vocalized in long *a*, see Tucker and Bryan 1966, 533-535)—at any rate these languages also seem to support the reconstruction of an *a* as the typical vowel of the imperfect in proto-Cushitic. This fits perfectly the pattern establishable for proto-Semitic and Berber, namely imperfect *yiqattVl*, perfect *yigtVl* where *V* is a thematic vowel variable according to the verb. This reconstruction (cf. my "La division des langues sémitiques" presented at the Congrès International de Linguistique Sémitique et Chamito-Sémitique, Paris, 1969) also makes *a* typical of the imperfect. The *i* and the *u* of the perfect in Cushitic may be the survival of the thematic vowel (especially the tri-radical Bedaaye root *ktm* seems to confirm this assumption).

## TOWARD A LEXICOSTATISTIC CLASSIFICATION OF ETHIOPIAN LANGUAGES

MARVIN L. BENDER

### 1. INTRODUCTION

This article is an abbreviated report on a project undertaken as part of the Language Survey of Ethiopia during 1968-1969 in an attempt to improve on our knowledge of the distribution and classification of indigenous Ethiopian languages.\*

The first step was collecting new data on the names, locations, and provenances of languages, and documenting them with new materials collected first-hand in the field. This task was carried out in the time available for an estimated 80-90% of all Ethiopian languages and was supplemented where necessary by reference to the older literature.

### 2. PRELIMINARY RESULTS: METHODS

Before attempting to set up the data for computer programming on all languages taken together, the languages for which data in the form of adequate 100-item lists was available were divided into seven sub-divisions for computation by hand. These sub-divisions are: Semitic—18, Cushitic (Oromoid—16), Cushitic (non-Oromoid—17), Omotic (Ometo—16), Omotic (non-Ometo—15), Nilo-Saharan I (Mabaan through Ingassana—13), and Nilo-Saharan II (Nilotic through Koman—18). The division reduces the number of individual comparisons for the 113 languages to approximately 86,500 from a total of 632,800 needed to handle all the languages taken together. In addition, approximately 10,800 additional comparisons were made for the case of 9 of the languages of Nilo-Saharan I against 12 of those of Nilo-Saharan II.

Results of the computations for the seven sub-divisions were arranged in tabular form, not given here, and correspondences between part of Nilo-Saharan I and part

\* I wish to acknowledge my indebtedness to the Language Survey of Ethiopia and its parent organization, The Survey of Language Use and Language Teaching in Eastern Africa. This work was done under Survey sponsorship and as part of the Survey's program.

of Nilo-Saharan II were also worked out. Of course the results are expressed in percentages in order to make them immediately comparable, since in many cases the number of comparisons possible is less than 100.

In the case of the first seven tables, suggested grouping diagrams were also drawn up to show how the tables can be interpreted to indicate the direction in which sub-grouping may proceed in the various sub-divisions.<sup>1</sup>

#### SUGGESTED GROUPINGS

1. ETHIO-SEMITIC (34)		
North (64)	Transitional (53)	South (52)
Geez- Tigrinya- Tigre	Amharic- Argobba Soddo  Gafat  Harari- Zway- Walani	Chaha- Geto- Gumer  Mesqan- Innemor  Indegegn

The numerical figures are minimal values of percentages found in the groups concerned. Insufficient data: Selti, Inneqor, Urbareg, Gogot, Muher, Izha, Iner, Gura.

2. CUSHITIC (non-Oromoid)			
North	Central (34)	East (10)	
		Highland (38)	Lowland (17)
Beja (5)	Awngi Qimant- Bilen (50) Xamta	Hadiyya- Maraqo (88) Kembata- Timbaro- Alaba (82) Sidamo- Derasa (63) Burji	Afar-Saho (66) Somali-Rendille (52) Baiso

The figures in parentheses are the lowest occurring for the grouping, except that the figure for Beja is the lowest occurring between Beja and all others.

<sup>1</sup> The details of the sub-grouping and some of the nomenclature are my own. It is assumed throughout that the major groupings are firmly established except for some questions of individual languages.

3. CUSHITIC (Oromoid)		
Galla (75)	Konso (28)	Werize (60)
Borena- Arusi- Qwottu (84) Guji Tulema- Mecha (90) Dasenech (16)	Konso-Gato (92) Gidole-Bussa (60) Arbore(?) (14)	Gawwada- Gobeze- Werize (68) Tsamai

The figures in parentheses are the lowest occurring for the grouping except that the figure for Dasenech is the lowest occurring between Dasenech and all others.

4. OMOTIC (Ometo)		
North (47)	East (57)	South
Welamo-Malo- Zala-Gofa- Gemu (88) Kullo-(Konta)- Dache-Dorze (71) Oyda Basketo	Zergulla- Zyse (86) Koyra- Gidicho (62) Gatsamba	Male (40)

The figure in parentheses for Male is the lowest occurring between Male and any other language.

5. OMOTIC (non-Ometo)			
Janjero	Kefa (23)	Maji (23)	Ari (43)
Janjero (4)	Kefa-Mocha (68) S. Mao Shinasha	Maji-Nao- Sheko (46) Bensho-She (Gimira) (79)	Dime Ari Banna-Hamer- Karo (75)

The figure in parentheses for Janjero is the lowest occurring between Janjero and any other language.

6. NILO-SAHARAN I				
SURMA (24)		NON-SURMA		
NUCLEAR (34)	NON-NUCLEAR	BERTA		
Mursi- Tirma Me'en Yidinit	Murle- Zilmamu Mesengo	Nara (Barya)  (15)	Wetawit- Gobato (60)  (14)	Ingassana  (14)

7. NILO-SAHARAN II				
NILOTIC-MABAAN (29)		KOMAN (5)		KUNAMA
NILOTIC (46) Anyuak- Shilluk Nuer Inyangatom (12)	MABAAN (47) Jumjum- Mabaan Burun	KOMA (19) N. Koma- S. Koma C. Koma- Uduk Langa N. Mao (14)	GUMUZ (77) Gumuz- Sese- Disoha- Other dialects	Kunama- Iilit  (8)

3. PRELIMINARY RESULTS: DISCUSSION

Discussion of Diagram 1. Zway and Walani are representatives of Eastern Gurage; Indegegn, Innemor, and Geto of Peripheral West Gurage; and Chaha and Gumer of Central West Gurage of the new classification proposed by Hetzron (1972, see also Ferguson e.a. 1971, section 1.1.2). Comparing this grouping with those usually given based on grammar and phonology, we see some similarities and some differences. The North group agrees with the other classifications and is based on the fact that no percentage less than 64 is found among Geez-Tigrinya-Tigre.

The Transitional group includes languages with correspondences not less than 53 among each other, but running as high as the 60's when the North group is compared with them and in the 40's as compared to the South languages. On the other hand, some of the Transitional group run only in the 40's with the North, but in the 60's with the South languages. In general, we can see that Amharic-Argobba-Soddo-Gafat run higher with the North languages and lower with the South languages. Harari-Zway-Walani run a bit higher with the South languages than with the North. But all the Transitional languages have fairly high comparisons with each other.

The South languages have no score lower than 52 with each other, but have scores down into the 30's with the North languages and one as low as 39 with the Transitional languages (Indegegn—Amharic).

The overall result is really quite similar to previous schemes. In all three cases, we have a North Group, a grouping of West Gurage languages (here called the South group), and a grouping of Amharic-Argobba with East Gurage in a larger grouping. The main difference here is that this latter grouping is not clearly a part of either North or South in this vocabulary correspondence scheme, but stands between them as a transitional or 'bridge' group.

There is little to be said at this time about Diagrams 2, 3, 4, and 5. Diagram 2 agrees quite well with the usually-accepted North, Central, and East Cushitic groupings. Diagram 3 falls outside the realm of comparisons because it deals mainly with the scarcely-documented languages southwest of Lake Ciarno for which sub-grouping has not previously been suggested as far as I know. Diagram 4 is in agreement with

previous classification and reinforces it by taking into account new and better data on some languages. As with Diagram 3, Diagram 5 deals with poorly-documented languages for which earlier attempts at classifications are few and sketchy. It supports the main lines laid out for example in Tucker and Bryan (1956): the distinctiveness of Janjero, the Kefa grouping, and the existence of at least two other groupings.

Comparing Diagrams 6 and 7 with the classification scheme of Greenberg (1966) we note the following major differences. First of all, vocabulary alone is not sufficient to make it clear exactly which clusters of languages constitute families, sub-families, groups, and sub-groups. For example, our vocabulary classification shows the two major divisions Nilo-Saharan I and II. As mentioned above, this is mainly a matter of convenience in carrying out the work of comparison. We do not claim that these divisions are of any genetic significance. Then we see that our next five divisions (Surma, non-Surma, Nilotic-Mabaan, Koman, and Kunama) include groupings at different levels in Greenberg's classification. That is, Surma is a group, Koman is a family, Kunama is a sub-family, and the other two divisions are not given by Greenberg's scheme as constituting any sort of genetic units.

The main difference in detail is that Jumjum-Mabaan-Burun seem to be closely allied to the Nilotic group, and vocabulary correspondences do not give a reason for setting them up at the family level alongside all of Chari-Nile or Koman.

#### 4. THE PROPOSED MASS COMPARISON PROGRAM

The main defect of the above fragmented comparisons is the lack of documentation of cross-grouping correspondences. For example, how can we be sure that comparisons between some Cushitic and some Omotic languages may not run significantly higher than figures within either family? In actuality, some of these computations have been carried out, and it is a fact that Cushitic-Omotic percentages do not exceed 12, and even percentages this high are found only between the languages in closest geographical proximity.<sup>2</sup> Still, percentages within some proposed groupings drop as low as 4% (Cushitic, non-Oromoid), 3% (Omotic, non-Ometo), or even 1% (Nilo-Saharan I and II). Furthermore, some interesting isolated high figures may be obtained between languages not currently in contact and not in the same families or even super-families. For example, Northern Mao shows surprisingly many cognates with the Ometo languages, though this may be due to historical Kefa contact with Northern Mao.

In any case, what is needed is the complete cross-table showing percentage figures between all pairs of languages from the complete set of languages of all super-families. If this data were available, it would be possible to be more confident of

<sup>2</sup> For a discussion of the influence of geographic proximity on basic vocabulary correspondences of languages, see Ferguson *e.a.* (1971), section 1.3.5.

those groupings based on percentage figures which do not exceed 10 or 20, e.g. Kefa, Maji, or Koman. This program has been carried out and analysis is now underway for publication elsewhere.

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#### DISCUSSION

LESLAU: A few questions. Somewhere it is said that there are 86 languages. Well, is a dialect such as Iner which is mentioned somewhere and which has very slight variations from Innemor a language? Or is Innemor (I speak about the Gurage group), which is very close to Indeegn, a language? Or is Indeegn, which is close to Chaha, a language? It is a very basic problem. I think it would be wrong to speak about 86 languages. I do not know myself what is a language and what is a dialect. In Semitic we count 8 languages. The Gurage cluster has 12 or 17 varieties—but certainly not 25 to 37 Semitic Ethiopian languages! Then there is the question of classification. I wish the Survey would give up any idea of classification because it will be extremely misleading. It should be abandoned because, regardless of what is said, that it should not be regarded as a gospel, it will have the title of "Survey of East African Languages" and every anthropologist will jump at it and be very pleased to see a very neat pattern that may fit into his ideas, and this will be perpetuated. And then the basic vocabulary. On the basis again of the hundred words that make up this table, I can play around with Gurage, taking one Eastern and one Western language—let us say Chaha as representative of the West, and Zway as representative of the East—and make every possible percentage of common vocabularies, intelligibility, and so on. So here again is something that should be avoided, one should not make any percentage or any comparison on the basis of a hundred words whatever they may be. Things are much more complicated than that when it comes to the vocabulary of languages. Even for the names of the parts of



the body or for kinship—basic elements—we see that closely related languages may have different roots. So there are basic problems.

PALMER: I really want to support what Professor Leslau has said, but for rather a different reason. If you want to see how suspicious we ought to be about the classification of Cushitic you should just look at the history of it. It started a long time ago, but certainly the first reasonable classification you find is in Reinisch who in fact divided the Cushitic languages up into two: the High ones and the Low ones, depending upon whether they were spoken in the mountains or not. So the classification begins based on a purely geographical feature and it only included the languages of the East—more or less those which Tucker has called the orthodox languages. You find however a detailed classification in Moreno, in a very tiny article published in 1938, and then a classified list in his *Grammar of Galla*, and a rather more detailed classified list in his *Manual of Sidamo*. And it is there that he divides up Low Cushitic into two and separates off Beja, which has stayed on its own ever since as far as I can see—it is stuck up there in the North and has never been associated very closely with any other language. And secondly he made this division of the Burji-Sidamo group, separating it off from the rest—a suggestion of this kind had already been made by Cerulli. At the same time he made the distinction that was mentioned a little while ago between the *ani* and *ati* languages and the *tā nē* languages. And this ended up with a classification into Northern, Eastern, Central and Western. Now Moreno gives almost no evidence at all as far as I can see for this, he just did this, added a few languages on like Konso which he says is certainly Low Cushitic without telling us why. It is therefore very disconcerting when you turn to Greenberg and you hear all about Moreno's careful review of the evidence. I would like to know where it is because I certainly could not find it. This to me more or less destroys the whole position. Greenberg in fact just added another group, the Southern group. He found a language, Mogogodo, which has got a certain number of words in common and stuck that on as well. He changed the Moreno classifications, in his Mogogodo he has got a more detailed classification which, as far as I can see, is not at all justified—I do not see any evidence for it and I am sorry to say this is the sort of thing that turns up again. It is based on Greenberg. Now whether there is independent evidence I do not know but the history of it makes me very suspicious, terribly suspicious, because the evidence is so lacking. It has never been presented, especially the Moreno evidence on which it is mostly based. And another thing that makes me terribly suspicious, as I said a little earlier, is this business of putting Beja right up on the corner. I know from the point of view of the vocabulary Beja does not tie in very well, whoever does the counting finds that there are very few common words. But my goodness anybody who knows any Cushitic at all need only spend about three minutes working on Beja to know that it is extremely like Afar-Saho or Bilin or the Agaw languages; in fact it is much more like those than it is like Somali and Galla and yet at the same time the classification does not reflect this at all.

HODGE: I do not really think we ought to be afraid of classification. I think that we should face the fact that these variants are there, that it is a question of, if you will, dialect geography: that you can make different classifications according to different facts; and it is just a matter of finding the facts and then if you want to come up with a classification which depends on vocabulary you do that; if you want a classification which depends upon certain morphological features you can do that, but it does not mean that you have one classification, any more than you have one way of dividing up dialects.

ANDRZEJEWSKI: I think that it is a very worrying fact that with all these classifications of the Cushitic languages, it is very often assumed that the material has been worked out, but it is not open to inspection. This is the case for example with the Afroasiatic section of Professor Greenberg's book. There is a list of words, and reference is made first to mass comparison, but unfortunately the actual mass comparison has never been published, only a very small number of items in fact, and if you try to find any confirmation of this it is very difficult to do so. The main point is this: once we abandon the concept of regular correspondence as the criterion for putting cognates together then our imagination can play us tricks, we have no scientific criteria for assessing similarity. This can be seen in Greenberg's list. I think that one would have to have a very vivid imagination to connect some of his 'cognates' and the same applies to Dolgopolskij's otherwise very interesting essay.

TUCKER: Just a small warning that came my way. For a long time in all the classifications I have seen that Konso and Geleba have been linked together—usually with a hyphen: Konso-Geleba or Geleba-Konso—and this seems to have been accepted uncritically by everybody. Well, when Miss Bryan and I were analyzing the languages we are calling Fringe Cushitic we came to the conclusion that Geleba was so aberrant as hardly to warrant the name Cushitic at all. Now that meant, should we now stick our necks out and say Konso-Geleba must go out? Fortunately we did not, we simply said that we do not know anything about Konso. And only a few weeks ago some information came in about Konso which showed undeniably that it is very close to Galla. So it is this accepting of somebody else's *ad hoc* classification as being authenticated which I think is so very dangerous.

ANDRZEJEWSKI: I think that anyone working on this kind of comparison, especially vocabulary comparison, and who publishes the results ought to make the basic raw materials available for inspection in some way ... if not published at least a microfilm should be obtainable. Because otherwise we simply do not know whether the assessment is reasonable or not. For example, in Reinisch's first book on the common origin of all the languages of the Old World, where he connects Indo-European with many African languages and Semitic, his lists of cognates really are attributable to his imagination.

BENDER: I must say that I also find a lot of Greenberg's comparisons very unconvincing and I think that Dr. Andrzejewski's criticisms especially are very valid. I think however you will find that I am extremely conservative about considering what things are cognates and compared to Greenberg on this I am almost a neogrammarian. With regard to publication I do plan to publish all the vocabulary items and all those which were considered cognates. So the whole mass will eventually be made available either as part of the *Monograph* or as a separate small publication and I think that will take care of that criticism. Professor Tucker talked about Konso and Geleba. This is one thing that we found, certainly Konso-Geleba does not hold together at all, Konso goes with the Galla languages, and Geleba—or Dassenech as it is actually called by its speakers—is really still somewhat a mystery but definitely does not belong together with Konso. Professor Palmer also mentioned the business about no evidence. I agree with him perfectly. Moreno does not give his evidence and I do not know where Greenberg got the idea that he did. But Fleming's article—which unfortunately is not here because Fleming himself did not come—does give quite a bit of evidence for the classification that we have adopted. Secondly I want to point out another thing. The classification that you find here in this scheme is not the vocabulary classification. It is based on Fleming's and Hetzron's work which involves morphological correspondence and so on. It is more like what people would consider a legitimate genetic relationship. However I think it is extremely interesting that if you do the mass vocabulary comparisons it falls very closely into line with what you get by looking at things in the more traditional sorts of way and—when you have the data—do the regular sound correspondences and so on, you find that you get a very good fit. Finally to go back to Professor Leslau's comments about the number of languages. Actually the reason this is here is that the question I get asked most often is "How many languages are there?". People are very curious about this, whatever it means. Now of course the problem of what is a language, what is a dialect, and so on is insoluble. But we did a study of mutual intelligibility within the Sidamo languages and there was another study done in Uganda by the Uganda Survey team on some Bantu languages. It was found that there is a linear relationship between basic vocabularies, correspondence and mutual intelligibility. So picking some sort of arbitrary figure which I chose to be 80%—the Uganda study chose 75, I am being more conservative—as the cut-off figure for basic vocabulary correspondence which separates language from dialect (of course this is arbitrary but the question of defining language and dialect is always arbitrary) on this basis and on the basis of what people say about mutual intelligibility and so on we arrived at this figure of 86. Now this is not at all unrealistic. First of all we do not say that there are 25 Semitic languages. For example Central West Gurage includes Chaha, Ezha, Gumer and Gura—these are all mutually intelligible and very close and all constitute one language as far as I am concerned. Peripheral West Gurage includes Indegegn, Iner, Innemor and Geto and these, as far as I know, are all mutually intelligible. So actually we end up with something like four dialect clusters

within Gurage and three separate languages besides which are Gogot, Soddo and Muher. And then the mass of the languages of Ethiopia are the Cushitic and the non-Cushitic languages. There are something like 30 Omotic languages, 20 Cushitic languages and about 28 or 30 of the non-Cushitic non-Semitic languages. And finally the question of the classification: I really do not understand this vehemence about publishing in the *Monograph*, with the endorsement of the *Language Survey*, the classification scheme proposed by Hetzron and Fleming.

ANDRZEJEWSKI: I think that what Dr. Bender said about the publication of his data is most welcome. I think that it is an important step forward.

CROSSLAND: Could Dr. Bender tell us something about the nature of the mutual intelligibility tests which he applied?

BENDER: We recorded brief passages in the languages in question—actually we had six stories recorded in each of six languages. Then we went to schools in the areas where these languages were spoken and played the stories to the children. We found sixth graders (average age 15 years) were best, and we gave them a modical choice test to be marked by pencil—everything was done in Amharic. The children had had experience using modical choice tests because the sixth grade are preparing for a test of this type which will be used to decide whether they go on to the seventh grade or not. Then we scored these tests and found that for students speaking language A, their scores in languages B, C, D, E etc., including their own language, were related directly to the degree of relationship of the languages in terms of the basic vocabulary overlap. It is a very striking result. It is a linear relationship and our results were very similar to what the Uganda Survey found.

## **VI**

### **CHADIC SECTION**



# THE HAUSA GENITIVE MORPHEME AS AN EXPONENT OF GENDER: A QUERY

NEIL SKINNER

The genitive morpheme in modern Hausa has been analysed by Parsons<sup>1</sup> as having three allomorphs, which he called 'zero', 'short' and 'long grades', and functioning as an exponent of gender in the following way:

	Zero	Short	Long
masculine	/-n/dn/ <sup>2</sup>	/na/	/naa-/
feminine	/-r/dn/	/ta/	/taa-/

So, for a masculine headword such as *gidaa* 'home', we get *gida-n sarkii* 'home-of king'; while for the feminine *'yaa* 'daughter', we have *'ya-r* (usually [*'yas*]) *sarkii* 'daughter-of king'. In thus functioning as an exponent of gender, the Hausa genitive morpheme contrasts, as Parsons pointed out, with the /n/ of Berber which follows headnouns of either gender.

If, however, we look closely at the zero genitive morpheme even in modern Hausa, we shall find a number of anomalies (many of them acknowledged by Parsons). Some of these occur even in the speech of younger speakers of the standard Kano dialect; but we shall find more if we examine other dialects. While, if we turn to older examples from written texts, the number of anomalies increases to such an

<sup>1</sup> Parsons (1960-1963). I understand from Dr. Kabir Galadanci, to whom I am indebted for the information and for reading through this paper, that Mr. Parsons has since preferred to keep the word 'grade' for the verbal set in Hausa. Dr. Galadanci adds that his own analysis of the genitive morpheme, made from the synchronic point of view, tackles the problem of the epicene nature (and varying tones) of the /-n/ by subdividing the genitive morpheme syntactically into two, according to whether it is followed by pronoun or by some other nominal; and, moreover, by positing that the /-n/ occurs as an allomorph of /-r/ after certain feminine nominals. From the synchronic point of view, this removes the matter from the category of anomaly or irregularity. The historical process behind the ambiguity of the genitive morpheme still, of course, begs explanation. I am also obliged to Dr. Paul Newman for several ideas which I have incorporated in this paper.

<sup>2</sup> /dn/ occurs after forms ending in a consonant, numerals and foreign words not yet properly assimilated into the language.

extent as to cast doubt on whether the /n/ can, in fact, even today be considered as a marker of non-feminine; while there are indications that once it was even less so.

The first of the anomalies in modern Hausa is that the genitive morpheme after any noun not ending in /-af-aa/ is always /-n/, whatever the gender of the noun. Admittedly the number of feminines not so ending is small and includes many borrowings; examples are *gwaggò* 'father's sister', *'en'è* 'native administration', *gwamnati* 'government', *'àraadù* 'thunder' and even such a basic item as *macèè* 'woman'. That this is not an automatic phonological process is shown by the occurrence of [ir C] and [ur C] and even, rarely, [or C] in other environments: e.g. *sirdii* 'saddle', *murnàa* 'joy', *bòrbor* 'gambolling'. Moreover, in the case of a headword ending in a consonant, which normally takes the *dn* allomorph, while there appears to be phonological conditioning to a certain extent—it is suggested that the [d] (< \*ʔ) is epenthetic—the other segment is never [r] and \**dr* is impossible. Though, if /r/ were the canonic form of the feminine genitive morpheme, one might expect it to occur. So we have *naš dn-kù* 'your (female) nurse'. Further, in such basic items as numerals, which function as feminines in other ways (e.g. *ukù taa fi biyu* 'three she exceeds two') the genitive morpheme is /-nfdn/ except in the case of *daya* 'one' when used with reference to a female. (It is normal for sex reference to affect gender in this way in Hausa). So *wadannan sun yi goomà dn/ goomàn wadāncan* 'these they make ten of those (i.e. are ten times as many)'.

When we turn from the standard Kano dialect to the speech of, say, Zaria, we find such sentences as *riigan sarkii taa keecèè* 'gown-of king she tore'. Here the speaker clearly recognizes the feminine gender of *rigaa*—witness the feminine personal pronoun *taa*—but uses /-n/ as genitive morpheme. It may be that this last is merely a comparatively recent development among Hausa speakers in peripheral areas, rather than an indication of a survival. Nonetheless it is evidence that the connection of the genitive morpheme with gender is the first to be weakened; might this itself not be evidence that the connection is not long established in the deep structure of Hausa?

Turning now to two corpuses of older Hausa, those of Schön (1885, collected in 1857) and Robinson (1896, but containing material written considerably earlier), thus adding some time depth to our perspective and, incidentally, predating the teaching of Hausa in schools with standardized textbooks using -r, the use of -n—at the expense of -r—greatly increases. Schön was a careful observer and used an orthography in most ways closer to phonetic than is the present standard spelling, though he failed to hear consonant gemination. Now, there are, as Parsons noted, no examples of -r in Schön's transcription of Dorugu's life story. But even today /-r C/ is most commonly realised as [-C C] with regressive assimilation, and it is probable that wherever Schön wrote Ø C, Dorugu had used consonant gemination, which Schön missed. So the title of the book, which today would be written *Maganar Hausa*, was by Schön written *Magana Hausa*.

Parsons speaks of feminine headwords in Schön's work as being quite arbitrarily



followed either by /-n/, by /ta/ or by Ø. Examples of the first are: p. 92 “*sabuwan duniya*”; p. 73 “*iyakan Hausa*”; p. 64 “*tukunyansa*”; p. 98 “*uwan gida*” ‘mother-of house, mistress’ (surely, if /-r/ was for him an exponent of feminine, Dorugu would have used it for such an uncompromisingly female word as ‘*uwaa*, though it may, paradoxically, have reference to males in certain important contexts); p. 65 “*kasuwam bayi*”; p. 27 “*maganam Bornu*” (in both the last two, note, Schön was scrupulous enough to write the labial rather than the alveolar nasal); and, lastly, from Dorugu’s *Life* (page not noted): “*riganka ... tana sauya ...*” where *rigaa* is formally feminine, witness “*tana*”, but once again the genitive morpheme is /-n/. So I suggest that the evidence points to the arbitrariness of the triple alternation as having been Dorugu’s, not Schön’s.

Turning to Robinson’s corpus, which includes poetry composed by Sultan Bello, that is dated perhaps thirty years prior to Dorugu’s telling of his life story to Schön, we find a similar situation, namely free variation in the zero genitive morpheme but, so far as one can tell, no -r. Confusion is added, of course, by the demands of metre and by the fact that the particular style of Arabic script used tends to make *n* and *r* somewhat alike and not dissimilar from *l* ([*l*] is often a dialect variant for [*r*]) and *y*, which is often used to indicate length in the preceding *a*! So, taking two feminine nominals that occur fairly frequently, we get (transcribing from the Arabic script):

<i>sarautansu</i>	<i>’ibaadaatai</i>
<i>sarautanta</i>	<i>’ibaadassu</i> <sup>3</sup>
<i>sarautatta</i> <sup>3</sup>	<i>’ibaadakka</i> <sup>3</sup>
<i>sarautalka</i>	<i>’ibaada tas</i>
<i>sarautaatai</i>	

To find an unequivocal genitive morpheme -r in a written text, we turn to Aliyu, Emir of Zaria’s poem about the gathering of chiefs in Kano,<sup>4</sup> composed in 1914, and there it occurs in the following sequence of varying genitive morphemes attached, note, to the same feminine nominal *makarantaa* ‘school’:

“*’akwai makarantan karatun Bature*” with /-n/,  
 (next line) “*’akwai makaranta ta Alkur’ani*” with /ta/,  
 (three lines on) “*makarantar Kano*” with /-r/.

However, it is not the phonological process of [*r* C] > [C C] and the fact that the two still co-occur as variants that is being considered here, but the status of /-n/ and /-r/ as exponents of gender, and the last example quoted brings us back to this point.

<sup>3</sup> The *tashdiid* (Hausa *Karfii*) or marker of consonant gemination is often not used in these texts. I have presumed it was omitted in these cases.

<sup>4</sup> Edgar Collection, Kaduna Archives.

Mischlich (1911, 20) writing at about the same time as this last poem, says of the *genitivpartikel* "Ist das vordere Wort männlich, so wird *na* (gewöhnlich abgekürzt zu *n*) angewendet, ist das vordere Wort weiblich, so wird *ta* (gewöhnlich abgekürzt zu *t* oder *r* in Kano und zu *l* in Sokoto) angewendet, sehr häufig aber auch *n* (in Kano)". That is, fifty years ago, even in Kano, it was not unusual to hear /-n/ as the genitive morpheme after a feminine nominal. May it not then be that at an earlier stage the genitive morpheme in Hausa was not gender-sensitive, but was /-n/ for masculine and feminine nominals indifferently? If this was so, it still remains to suggest how /-t/ came to be an allomorph of the genitive morpheme.

To form some idea of how this might have occurred, let us leave for the moment the genitive morpheme and its function as an exponent of gender and consider the question of the formal marking of gender within the nominal itself. Here again we meet with a strikingly paradoxical situation, that whereas the 'normal' marker of feminine is a suffixed /-af-aa/ (or, often, /-iyaa/, /-uwaa/ or /-niyaa/), there are some twenty words from the basic Hausa inventory, ending in /-aa/ that are masculine, including *gidaa* 'home' (which Parsons suggests is a plural form), *suunaa* 'name' (for which Lukas hypothesises \* < *sum-naa*), *sauraa* 'remainder' and a number of others such as *wàasaa* 'play', *sàndaa* 'stick' and *iskàa* 'wind, spirit' which are masculine in one or other of the main dialects. On the other hand, if a word is taken into the language, such as *àlbasàa* 'onion' (from Arabic) or *mootà* 'motor-car' (from English), an ending in /-aa/ usually seems sufficient to ensure feminine gender for the word (unless the referent is male), while any other ending makes the word masculine (unless its referent is female), except in certain fairly clear cases of analogy or ellipsis. That is to say, the /-aa/ ending is NOW a normal marker of feminine, but it seems that it was once not so.

Turning to verbal nouns and their gender, we find that, in Abraham's words, they "can be either masculine or feminine, but are usually treated as feminine". However, it will usually be found that ambiguity of gender is much more likely to appear in the genitive morpheme than in the pronoun referring to the verbal noun. Primary verbal nouns, which almost invariably end in /-aa/, may in fact be followed by either /-n/ or /-r/, but the pronoun referring to them is usually feminine. Whether they have come to be treated as feminine, since the emergence of /-aa/ as a feminine marker, through analogy; or whether they are so for some other reason, perhaps similar to that which causes numerals and adverbial nouns, such as *sànnu* 'slowness' and *gòobe* 'tomorrow' to be feminine, is hard to say.

However, though /-aa/ may formerly not have been a marker of feminine, it seems likely that—with the exception of *macè* (? < \**matàa*)—there are no feminine items of the basic lexicon that end in anything but /-aa/. If then the formal marking of gender within the nominal is not something that has developed later in the language (perhaps under the influence of the large number of loanwords from Arabic) it seems possible that it may once have been something other than /-aa/. A consideration of feminine markers in other Chadic languages and Berber—to go no wider afield—

and, indeed, in the Hausa pronouns, makes /t/ an obvious consonantal choice to try first.

Now, if the feminine suffix in Hausa was once /\*-at/, similar to the Berber /-t/, we should have an explanation for the fact that, even today, only (feminine) nominals ending in /-aa/ can add the /-r/ (< /\*-t/) genitive morpheme; and, for example, 'ya-r sarkii would be a reflex of \*'ya-at n sarkii > \*'ya-t sarkii. To illustrate this hypothesis graphically:

Stage I			
m. <i>gidaa</i>		<i>n</i>	<i>sarkii</i>
f. 'ya	<i>at</i>	<i>n</i>	<i>sarkii</i>
Stage II			
m. <i>gidaa</i>		<i>n</i>	<i>sarkii</i>
f. 'yaa	<i>t</i>		<i>sarkii</i>
Stage III			
m. <i>gidaa</i>	<i>n</i>		<i>sarkii</i>
f. 'yaa	<i>t</i>		<i>sarkii</i>

If [*\*-t-n*] > [*\*-t*] at Stage II, the /-n/, having disappeared after feminine nominals, might be felt to be a marker of masculine. An interesting contrasting development in Tamazight may be noted here.<sup>5</sup> In that language the /n/ genitive morpheme regularly causes the following /t-/ prefix of a feminine nominal to assimilate, but itself often assimilates to [i-] or [u-] initial to masculine nominals. Thus for some speakers /n/ has come to be felt as linking only with following words that are feminine! For the survival of the suffixed [-t] there is considerable evidence, including -t-sa from the Krause corpus, and similarly, its exclusive use after feminine nominals in -t-a 'my' and, with some older Kano speakers, ta-sà 'his' (? < \*-tn-sà), -ta-tà 'her' and -ta-sù 'their'.

A random search in modern Hausa for evidence that /-at/ may once have been a marker of feminine does not yield much evidence in the shape of survivals. A number of ideophones and the numeral 'five' end in /-at/, but, so far as I know, other than a number of not yet completely assimilated loanwords from English and French such as *kwat* 'coat' (likely to go to \**kwatì* > \**kwaci*) there are no nominals which still end in this way. Nor should we expect to find any, since it would be contrary to the present phonology of the language and, in any case, the hypothesis is that the /t/ segment of the suffix is still in existence, but with a shifted function. However, there is one interesting word, *kyat*/*kyar*/*kyar* which Bargery (1934), s.v. categorises as a noun and which is generally considered to be a free form, but which does not seem to occur nowadays except with a preceding *dà* 'with'. So *dà kyat* 'with difficulty'

<sup>5</sup> I am indebted to Dr. Jeanette Harries for this information.

but 'difficulty' is not *kyat* but *wùyaa* (f.). Here again a comparison with Tamazight is illuminating, where a verbal *u'ar/ tu'ar* 'to be difficult' occurs, giving a feminine noun *t-ix-t* 'difficulty'. If the analogy has any validity, *kyat* might preserve for us an older form of feminine nominal which has escaped change by virtue of the fact that it occurs only in a single, invariable phrase, which could never, for example, be followed by the genitive morpheme. The fact that verbal nouns and some numerals and adverbial nouns ending in */-aa/* or */-a/* do not invariably select */-r/* even though feminine might be a reflex of their never having ended in a suffixed */-at/*.

The somewhat speculative nature of these thoughts about the genitive morpheme in Hausa should not hide the fact that they are an attempt to propose one solution that would solve two problems: (1) the occurrence of the */-af-aa/* as a marker of feminine in most nominals, but not in a significant number of items from the basic inventory of the language, and (2) the puzzling fact that the */-t/-r/* genitive morpheme, which obviously has some connection with feminine, is not used in a number of cases after feminine nominals, though phonologically there is no objection to its use. A comparison with the genitive morpheme and with feminine marker suffixes to nominals in other Chadic languages, to see whether there is support for the suggestion of an epicene */-n/* genitive morpheme and an */-at/* feminine suffix at an earlier stage would be interesting. If there is not, we might have here an indication—and there are others also in items of basic vocabulary—that Hausa is in some ways closer to Berber than to other Chadic languages.

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#### DISCUSSION

PARSONS: I entirely accept my friend Neil Skinner's findings. It has solved one of the little problems and, incidentally, brought more affinity between Hausa and Berber. But there are certain implications which he has not followed up. The two exponents

of gender which are exotic by European standards are that in the genitive morpheme (or copula or genitive case particle) and that in what can be called the identity particle (or stabilizer) and/or the copulative 'verb' 'to be'. I had attempted in the series of articles on gender in Hausa to link these and I made what I admit was a completely bogus attempt to link these two things semantically by calling the genitive copula a quasi-Indo-European participle meaning 'pertaining to' and the other, *ne/ce*, which could be vaguely associated with the effective or grade IV of the verb indicating complete action, would then mean 'pertaining to the point of identity'. But I would like publicly to withdraw that attempt to link these two and now, in the light of Mr. Skinner's paper, entirely divorce the two things. As to the origin of the *ne/te*, from earlier *na/ta* preserved dialectally, my hunch is that it is a deictic. In other words when you say *bako ne* 'a stranger (he) is, he is a stranger' you are really saying 'a stranger he' or even 'I'; for, after all, *n* we have seen is not confined to any one person. It is significant that with a genuine personal pronoun you have three possible structures—the standard structure is *ni bako ne* 'I am a stranger'; the marked structure or rather one marked structure is *ni ne bako* 'I'm the stranger', where 'the stranger' is the known element; and finally you do get very frequently in colloquial Hausa, but only with the personal pronoun and a demonstrative *wannan*, *bako ne ni* which stresses the complement *bako* 'a stranger am I', which if you like is really 'a stranger I I'.

PRASSE: Mr. Skinner's paper has confirmed a suspicion which I have had for some time, namely that the feminine form of the genitival particle in Hausa, the *ta*, must be secondary. This is an excellent example of just how complicated the situation can become when one refuses to accept mere resemblance as a proof and proceeds to an analysis of each phoneme of a word and account for each difference and resemblance. This Hausa *na* has been equated with the Berber and Egyptian genitival particles and it is Egyptian which would appear to have the most archaic form of it. In Old Egyptian it seems to be an adjective formed from the preposition *n* by means of the *nisba* ending. Even if the *nisba* ending *-i* itself is poorly attested in Old Egyptian, it is clear that the particle is declined; it has the feminine form *net* as well as a plural form. Now what has become of this ending *-i* in Berber and in Hausa, if this is the original form? Neither in Hausa nor in Berber, it seems to me, can it be reduced to a form *\*ni*: Hausa has *na* or *nā*, and the same seems to be the case in Berber. The Berber form, at least with a suffix pronoun, seems to end in an *a*—that is to say, with the pronoun of the second person, *annak*. Secondly, there is a syntactic problem: in Egyptian you can only have this particle *ni* with a noun as complement, and this noun is what we would call in Semitic philology a limitative complement, the same kind as we find in expressions of the type 'beautiful of face'. But you cannot have it with a suffix pronoun. Now in Berber and in Hausa apparently you CAN have it with a suffix pronoun. Thirdly, there is the problem of the gemination which you find in Berber, for it is clear that in all the Berber dialects you have gemination of the *n* at least with

a suffix pronoun, as in the form *ənnak* which I have just quoted. But there are at least some Twareg dialects which also have the gemination simply in intervocalic position between nouns. I should finally like to draw attention to the fact that there may be a connection between a genitival particle and a preposition not only in Egyptian but also in Berber. Already in Egyptian you can say that in a certain way the preposition *n* plus a suffix pronoun forms some kind of a parallel to the syntagm *ni* plus a nominal complement, like in expressions such as *imi ni* 'belonging to me'; and in Berber we have in some eastern dialects a preposition *in* which means 'towards'. But you have it also in Twareg in the conjunction *ininnin* which seems to be made up in the same way as *full-innin*, and *s-innin* and *d-innin*. And perhaps you can find this prepositional form also in the first person, where you say apparently in most Berber dialects *-inu* 'mine'. Perhaps this *in-* is not to be directly compared with the *ənn-* of *ənnak*, as in Kabyle we have a whole series of suffix pronouns with *in-* in the singular: you can say *inək*, *inəs*, etc. and these pronouns need not have a syntactic head, they constitute a genitive without a head, if you like.

JUNGRAITHMAYR: A few remarks from the point of view of the other Chadic languages. Mr. Skinner says in his last lines that a comparison with the genitive morpheme and with feminine marker suffixes to nominals in other Chadic languages, to see whether there is support for the suggestion of an epicene */-n/* genitive morpheme and an */-at/* feminine suffix at an earlier stage, would be interesting. Well so far we cannot say anything definite, but a few languages spoken in Northern Nigeria in the vicinity of Hausa show the distinction of a nasal element for genitival construction for what we used to call masculine and a *t-* element for the feminine. In the Ron languages the formula is that masculine and plural generally have a basic consonantal element *m*. The situation is reflected in the five following languages as follows. Daffo Ron on the Southern Plateau would have *ma* for masculine, *ti* for feminine, and again *ma* for plural—without differentiation of gender; Sha, the neighbouring language, would have *ma*, *ti* and *mi*; Kulere, the next neighbouring language, would have *ma*, *muka* (a possible derivation from *ma*), *mu*; more reduced reflexes of these seem to me to be those of the geographically next languages like Bokkos where instead of *ma* you have an *'i* but *ti* being retained for feminine singular, and plural again *'i*. Now, if Hausa had a three stage development as outlined by Mr. Skinner we would first have to consider the fact that Hausa shares a feature with at least a number of other Chadic languages, so that when the presumed proto-form of Hausa separated from that which was proto-Berber, at that time the Chadic languages by developing this difference of *n/t* must have already formed some kind of a common linguistic group. We will later have the opportunity of discussing Mr. Parsons' paper in which he tends to link Hausa structure not with the other Chadic languages but with the North, be it Berber or Egyptian. We will then I think have to ask the question: "Since Hausa shares this feature with the other Chadic languages, or at least with a certain number of them, at what stage could this separation have taken place?".

SKINNER: I am very grateful to Dr. Jungraithmayr for that information but I would also like to know whether there is evidence of an *-at* suffix in other Chadic languages, which is I think also relevant to the question.

CARNOCHAN: I would like to make three remarks on this question. The first point concerns the link between the *r/n* of this so-called genitive relationship and the deictic particles at the ends of words. For instance one has *rigar sarki* 'the gown of the chief' but one also has *rigar* 'the gown that we have been talking about'. Now these are differentiated by particular pitch features but certainly as far as the phonetic noises are concerned you again have the nasal on the one hand and the rolled *r* on the other, the rolled *r* being limited to feminines ending in *-a*, and the rest having the nasal. The second point is that we have seen that there are these distinctions from dialect to dialect in so far as the nominal piece is concerned—that is to say that certain dialects do not recognize the category of gender with masculine and feminine forms. But are there any dialects which do not recognize gender within the verbal piece, which do not distinguish *ta zo* from *ya zo*? The third point is that we have a quite different relationship between nouns where these are of the personal name type. For instance we have *sarkin Kano* but we do not have *\*Amadun Kano*—or do we? We have *Amadu Kano*, but does this mean 'Amadu of Kano'? or 'Amadu from Kano' or is it 'Amadu the Kano man'? At least in this sort of situation we seem to have an entirely different relationship of nominal to nominal within the piece or indeed you may say that these are in apposition and there are two nominal pieces in such a structure.

PARSONS: With regard to the first point, I think that there can be no doubt that there has been loss of the deictic *naŋ*, and I had hoped that Mr. Skinner was going to follow up a thought he just gave me that the deictic *naŋ* and *čaŋ* are part of the same thing, the *č* goes with femininity you see. The tonal behaviour that Mr. Carnochan observed is all accounted for if you re-introduce the word *naŋ* with a single mono-tone which indicates 'the one under reference', as opposed to *naŋ* 'the one coming here, present, towards me'. A parallel, of course, is that the article in most Indo-European languages is a worn down form of the demonstrative. Loss of the *ya-/ta-* of verb occurs only in what you might call the most debased Hausa dialects such as that spoken in Bauchi—although by monoglots (who also do not glottalize and will say *dan seriki* like an Englishman). Those sort of people will say *matanši ya-mutu* 'his wife has died'. The third point is very interesting because a student of ours now working with Mr. Skinner, M. K. Galadanci, wrote a thesis on the nominal group in Hausa in which he singled out personal names for their special limitations and this was one of them. You can certainly say *Auduŋ* 'the particular one we were talking about', but as far as I remember he said that you could not say *\*Audun naŋ*.

JUNGRAITHMAYR: One remark on the question of loss of gender distinction. It is



interesting to note that certain Chadic languages have given up the distinction of gender in the prefixal conjugation but have kept it in the suffixal conjugation with intransitive verbs. For instance with the verb *mutu* 'to die', which is *mur* in Dera, 'he died' would be *amureni* and 'she died' would be *amureto*, so that *-ni/-to* would probably represent the older stage of opposition whereas the prefixal conjugation is simplified into a single element, *a-*.



## TYPES OF CONJUGATIONAL FORMS IN CHADIC

H. JUNGRAITHMAYR

0.1. The purpose of the present paper is twofold: (1) to present data, much of it previously unpublished, and to make it generally accessible by displaying it paradigmatically, (2) to suggest ways of comparing conjugational forms within the Chadic (sub-)family of Hamito-Semitic. The paper is, accordingly, divided into two parts: discussion and documentation (in the appendix). First, a few methodological and technical remarks are necessary.

### *Preliminary remarks*

0.2.1. According to the lists published by Greenberg (1966) and Voegelin (1964), the number of languages which we know of to date which are generally ascribed to this (sub-)family total approximately 115. They are spoken in Northern Nigeria, Southern Niger, Northern Cameroons, the central part of the Chad Republic and a few in the border region of the Central African Republic. So far basic information on some 30 to 40 of these languages has been gathered, although only part of it has actually been published.

0.2.2. The brief discussion which follows is based on data drawn from 15 languages. The reason for their selection was simply the fact that these were the ones to which I had best access since the data concerning them was in my own files. With the exception of Mubi (Chad) all are spoken in Northern Nigeria. Thus, while any results derived from this data may have some general validity for the Western Chadic languages, this is certainly not necessarily so for the entire family.

0.2.3. Apart from Margi, all the languages presented have been classified as Chado-Hamitic (or 'non-class languages') in the *Handbook of African Languages* (Westermann and Bryan 1952) and as belonging to the Plateau-Sahel group by P. Newman (Newman and Ma 1966). This discussion may, therefore, by concentrating on problems of one 'group' of the sub-family, neglect vital aspects of the other 'group'

(namely the 'Tschadisch/Chadic languages' in the narrower sense of the *Handbook* or the Biu-Mandara languages of Newman).

0.2.4. In displaying the conjugational forms of a particular language an attempt has been made to reduce the number of paradigms to those which appear to be morphologically basic (thus excluding secondary derivations) or which seem to be required from a comparative point of view. Thus, for example, Hausa is represented by only four paradigms, namely those of the aorist-jussive, the relative past, the habitual and the progressive, whereas the two future tenses, the perfect and the relative progressive have been excluded.

0.2.5. In addition to the conjugational forms, the possessive suffix bases have been added for each language because it was felt that these often contain valuable information regarding the history of the pronouns in a language; that is to say they often preserve earlier stages of development within a pronominal system (cf. Tangale, Pero, Ngamo).

0.2.6. The morphological and semantic transformation of conjugational forms in any given language is a continuous process. It must not, therefore, *a priori* be expected that historically related forms in presently spoken languages will necessarily display identical or almost identical morphological features and/or semantic characteristics. This does NOT, of course, preclude the possibility of genetic relationship being involved where great similarity of shape and meaning between forms is present in languages which have been separated for some considerable time. Every given language community makes its own use of its linguistic heritage. A practical implication of this is that what may be labelled, for example, 'the present tense' in a description of one linguistic system is not necessarily comparable to 'the present tense' of another but perhaps rather to its 'habitual form'. Thus the Tangale 'habitual', formed by means of the auxiliary *gán* (e.g. *na gán sélii* 'I usually beg'), is comparable to Hausa *ná kàn ròðkáá* but is not, however, comparable to the Bolanci 'habitual forms' in *-o*. Forms in *-o* do exist in Tangale also, but differently 'grammaticalized' and/or labelled, namely as a progressive form (though with habitual tendencies, e.g. *naɲ selo* 'I am (always) begging'; cf. also Jungraithmayr 1968b, footnote 6).

### *Discussion*

1.1. The languages selected for this discussion are: Hausa (standard), Angas (Kabwir), Sura (Panyam), Fyer-Ron, Daffo-Ron, Bokkos-Ron, Sha-Ron, Kulere-Ron, Dera/Kanakuru (Shelen), Tangale (Kaltungo), Pero (Filiya), Ngamo, Karekare, Mubi and Margi. Material on Ankwe (Goemai), Wurunkun and Bolanci (Potiskum) was also available but did not yield sufficient information on this issue; they are, however, referred to occasionally.

1.2. In each of these languages a distinction between two markedly different conjugational forms, a basic or non-extended form and an extended form (so to speak), seems to be fundamental. The basic form appears in the languages under consideration as 'aorist' and/or 'jussive(-subjunctive)', whereas the extended form has either been grammaticalized as 'progressive' and/or 'habitual' ('present-habitative'); see the following table and the appendix:<sup>1</sup>

	NON-EXTENDED		EXTENDED	
	Aorist	Jussive	Habitual	Progressive
(1) Hausa	<i>(ká cí)</i>	<i>kà cí</i>	<i>ká kàn cí</i>	<i>ká nàà cíl</i>
(2) Angas	<i>ya se</i>	<i>ya se</i>		<i>ya pò sé</i>
(3) Sura	<i>woya se</i>	<i>ya se</i>		<i>woya pù sé</i>
(4) Fyer	<i>há 'et</i>	<i>hà 'èt</i>	<i>haá 'ét</i>	<i>haâ 'ét<sup>2</sup></i>
(5) Daffo-Ron	<i>há cuh</i>	<i>hà cuh</i>	<i>há cwaáh</i>	<i>há má cwayì</i>
(6) Bokkos-Ron	<i>há cu</i>	<i>hà cu</i>	<i>há cwááy</i>	<i>há má cwayì</i>
(7) Sha-Ron	<i>yí ci</i>	<i>yí ci</i>	<i>yí cáyây</i>	<i>ya má cíi</i>
(8) Kulere-Ron	<i>yá ci</i>	<i>yá ci</i>	<i>yá cááy</i>	<i>yá cááyé</i>
(9) Dera	<i>ka twí</i>	<i>ka twí</i>	<i>kái twa</i>	<i>kai túmái</i>
(10) Tangale	<i>ka saa</i>	<i>ka saa</i>	<i>ká gán saa</i>	<i>kañ saa-ni</i>
(11) Pero	<i>ké cí</i>		<i>ké ñaŋ cí</i>	<i>kí kó tá cwááni</i>
(12) Ngamo	<i>kò tù-</i>		<i>kò tìl-shê</i>	<i>kò zúk tìl-shê</i>
(13) Karekare	<i>ká tú-</i>		<i>ká tee-káu</i>	<i>káá tú-nà</i>
(14) Mubi	<i>ká tìl</i>		<i>ká túwà</i> (‘present’)	
(15) Margi	<i>*nàgè wì</i>	<i>. . g-á-wì</i> (‘Sub- junctive’)	<i>nàg-á-wì</i> (‘present’) <sup>3</sup>	<i>nàg-á-vàr wì</i>

1.3. The ‘extension’—or some other modification, for example of the tonal pattern of the preceding person-tense-marker—may concern either the pronominal complex or the verbal complex or both; e.g. Hausa *ká cí* vs. *ká-kàn cí*; Bokkos-Ron *há cu* vs. *há cwááy*; Dera *ka twí* vs. *kái twa* or *kai túmái*, Tangale *ka saa* vs. *kañ saani*.

1.4. Semantically, these opposing forms, non-extended vs. extended, may best be described in terms of ‘perfective’ vs. ‘imperfective’, or perhaps rather ‘constative’ vs. ‘cursive’ since the former terms are generally employed at the lexical level. I have

<sup>1</sup> The verbal example in all languages is ‘to eat’ except in Margi where *wì* ‘to run’ is used. The forms are quoted in the 2nd person singular masculine.

<sup>2</sup> Problems regarding the semantics of the progressive and habitual tenses are discussed in Jungraithmayr (1970), § 173.

<sup>3</sup> Cf. the semantic description of this present tense in Hoffmann (1963), § 316.

discussed this problem at some length in earlier articles (see the relevant entries at the end of this paper). As to the position ('*Stellenwert*') of the perfect tense—which I sometimes wrongly labelled 'perfective'—within the aspectual systems of the languages in question, my view differs today from that which I expressed in these earlier articles. The conjugational forms of the perfect are, as a rule, derived from the respective forms of the aorist, either by *-ń/-á* as in Hausa, by *-à* as in Angas, by *-go/-ko* as in the Bolanci-Tangale group, or by *tì-* as in Bokkos-Ron, etc.

1.5. For the Ron languages, as well as for Angas and Hausa, I have suggested—and I wish to repeat the suggestion in the present larger context of fifteen languages—that such a contrasting pair of forms (i.e. non-extended 'aorist-jussive' vs. extended 'habitual', 'present-habitative', or sometimes 'progressive') be considered fundamental to the conjugational systems of the languages discussed here—and perhaps even of the entire Chadic family.

1.6. Languages like Ron and Mubi appear to have preserved best this fundamental aspectual system by displaying contrastive forms like:

Kulere: *yá ci* 'you eat, ate, should eat' vs. *yá cááy* 'you usually eat' (also extra-temporal present);

*yá syel* 'you hear, heard, should hear' vs. *yá syáláy* 'you usually, habitually hear';

Daffo: *há mot* 'you die, died', (vs. *há mot* 'you should die') vs. *há mwaát* 'you usually die, faint';

*há lifit* 'you stand/stood up' vs. *há lifyaát* 'you usually stand up';

Mubi: *ká tìl* 'you ate' vs. *ká túwà* 'you (usually) eat'

*ká wècik* 'you danced' vs. *ká wicáák* 'you (usually) dance';

*ká beeni* 'you built' vs. *ká binnáá* 'you (usually) build'.

1.7. In certain other languages like Dera (Kanakuru), the 'synthetic' extension by means of an internal *-aa-* has been reduced to a suffix *-a*, e.g.:

*ka twí* 'you ate, have eaten, should eat' vs. *kái twa* 'and you eat, ate';

*ka shení* 'you remembered, should remember' vs. *kái shená* 'and you remember(ed)';

cf. also Karekare *ká tú-* vs. *ká tee-*; Jegu *kée tè* vs. *kée tá*, etc.<sup>4</sup>

1.8. Several languages, including Hausa, Angas, Sura and Tangale, have either lost or have only partially preserved those primary features of the extended forms

<sup>4</sup> Whether Margi *nàg-á-wí* represents such an extended form in *a* cannot yet be decided. The semantic features of this 'present tense' would very much support such a view.

and have developed instead new and individually differing forms: e.g. Hausa *ká kàn cí*, Tangale *ká gán saa*, Pero *ké ɗaɗ cí*; Hausa *ká nàà cìl*, Angas *yá pò sé*, Dera *kai túmái*. These innovatory forms of the habitual and progressive tenses are constructed analytically from auxiliary particles and/or verbal nouns and/or disjunctive personal pronouns respectively. It was Klingenheben who first drew attention to the probable Benue-Congo origin of the auxiliary particle *kàn* in Hausa.

2.1. If we follow the practice of Iranian linguistics, as proposed by Diakonoff (1965, 11), we might interpret the state of affairs encountered in some of the Ron languages as a “partial remodelling of the system of morphology” thus ascribing them to his ‘middle stage’ of development. On the one hand these languages have a dual system of aspects with a ‘constative-cursive’ contrast, on the other hand they have also developed a number of ‘tenses’ (*Aktionsarten* and *Tempora*). Other languages, like Angas, Sura and Hausa, do not show remnants of that basic aspectual system but rather display a “complete reshaping of the system” and thus belong, at least in this respect, to Diakonoff’s ‘new stage’ of development.

## APPENDIX

*Conjugational forms in 15 Chadic languages<sup>5</sup>*

## 3.1. HAUSA

(ci/cíl ‘to eat’)

	Aorist: Jussive	Rel. Past Narrat. ‘Aorist’	Habitual	Progressive	Poss.
sg. 1st	*ná:’ñ cí	ná cí	ná kàn cí	’ñ nàà cíl	-á
2nd m.	*ká:kà	ká	ká	ká	-kà
2nd f.	*kí:kí	kíkà	kí	kí	-kí
3rd m.	*yá:yà	yá	yá	yá	-sà
3rd f.	*tá:tà	tá	tá	tá	-tà
pl. 1st	*mú:mù	múkà	mú	mú	-mù
2nd	*kú:kù	kúkà	kú	kú	-kù
3rd	*sú:sù	súkà	sú	sú	-sù

Note that Parsons terms the Hausa relative past ‘aorist’ whereas what Gouffé calls ‘aorist’ is what we are used to call subjunctive or jussive, while Klingenheben calls both the *sun* and the *suka* forms ‘aorist I’ and ‘aorist II’ respectively.

<sup>5</sup> Tones are marked in the following way: à, àà low, á, áá high, a, aa mid, á, áà high-low falling, áa high-mid falling, áá mid-high rising, etc.

3.2. ANGAS (*se/sé* (VN) 'to eat')

	Aorist: Jussive	Future	Perfect	Progressive	Poss.
sg. 1st	<i>ɲa:ɲà se</i>	<i>ɲá se</i>	<i>ɲa-à se</i>	<i>ɲán pɔ́ sé</i>	- <i>na</i>
2nd m.	<i>ɣa:ɣà</i>	<i>ɣá</i>	<i>ɣa-à</i>	<i>ɣà</i>	- <i>ya</i>
2nd f.	<i>yí:yì</i>	<i>yí</i>	<i>yí-l</i>	<i>yì</i>	- <i>yì</i>
3rd m.	—:( <i>nyi</i> )	<i>ká</i>	<i>kà</i>	<i>nyi</i>	- <i>ni</i>
3rd f.	—:( <i>nyi</i> )	<i>ká</i>	<i>kà</i>	<i>nyi</i>	- <i>ni</i>
pl. 1st	<i>mu:mù se</i>	<i>mú se</i>	<i>mu-ù se</i>	<i>mún pò sé</i>	- <i>nu</i>
2nd	<i>wu:wù</i>	<i>wú</i>	<i>wu-ù</i>	<i>wún</i>	- <i>wu</i>
3rd	<i>mwá:mwà</i>	<i>mwáá</i>	<i>mwáà</i>	<i>mwá</i>	- <i>kámwá</i>

3.3. SURA (*sɛ/sé* (VN) 'to eat')

	Aorist: Jussive	Relative Past	Progressive	Poss.
sg. 1st	<i>wañ/'án: dto se</i>	<i>kân se</i>	<i>wañ pù sé</i>	-( <i>a</i> )- <i>n</i>
2nd m.	<i>wɔɣa/ɣa: 'a</i>	<i>'aà</i>	<i>wɔɣa</i>	-( <i>a</i> )- <i>k</i>
2nd f.	<i>wii/yi: yì</i>	<i>yil</i>	<i>wii</i>	-( <i>i</i> )- <i>k</i>
3rd m.	<i>(wu)rí: rí</i>	<i>(wu)ríl</i>	<i>wurí</i>	-( <i>i</i> )- <i>n</i>
3rd f.	<i>(wù)rá: rá</i>	<i>(wù)ráà</i>	<i>wurá</i>	-( <i>a</i> )- <i>t</i>
pl. 1st	<i>mú: mu</i>	<i>muù</i>	<i>mu</i>	-( <i>u</i> )- <i>n</i>
2nd	<i>wú: wu</i>	<i>wuù</i>	<i>wu</i>	-( <i>u</i> )- <i>k</i>
3rd	<i>mɔ́: mɔ́</i>	<i>mɔ́ɔ́</i>	<i>mɔ́</i>	-( <i>u</i> )- <i>t</i>

Note that there are still other auxiliary particles besides *pù* in the progressive, e.g. *ḡùṇ*, *ká ñ*.

3.4. FYER-RON ('*et/'ét* 'to eat')

	Aorist: Jussive	Perf.: Past	Habit.: Prog.	Poss.
sg. 1st	<i>yí:yì 'et</i>	<i>yáà: yí kà 'ét</i>	<i>yaà: yaà 'ét</i>	- <i>in</i>
2nd m.	<i>há:hà</i>	<i>háà: há kà</i>	<i>haà: haà</i>	- <i>o</i>
2nd f.	<i>ší:šì</i>	<i>šáà: ší kà</i>	<i>šaà: šaà</i>	- <i>ish</i>
3rd m.	<i>mí:mì</i>	<i>máà: mí kà</i>	<i>maà: maà</i>	- <i>is</i>
3rd f.	<i>tí:tì</i>	<i>táà: tí kà</i>	<i>taà: taà</i>	- <i>it</i>
du. 1st	<i>kú:kù</i>	<i>kwáà: kú kà</i>	<i>kwaà: kwaà</i>	- <i>uk</i>
pl. 1st in.	<i>ká:kà</i>	<i>káà: ká kà</i>	<i>kaà: kaà</i>	- <i>akan</i>
1st ex.	<i>yí:yì 'et nyí</i>	<i>yáà: yí kà 'ét nyí</i>	<i>yaà: yaà 'ét nyí</i>	- <i>anyi</i>
2nd	<i>wú:wù</i>	<i>wáà: wú kà</i>	<i>waà: waà</i>	- <i>u</i>
3rd	<i>sú:sù</i>	<i>sáà: sú kà</i>	<i>miná: mináà</i>	- <i>us</i>

3.5. DAFFO-RON (*cuh* 'to eat')

	Aorist: Jussive	Habitual	Progressive	Poss.
sg. 1st	<i>yí:yì cuh</i>	<i>yí cwaáh</i>	<i>yí má cwayì</i>	- <i>in</i>
2nd m.	<i>há:hà</i>	<i>há</i>	<i>há</i>	- <i>a(h)</i>
2nd f.	<i>ší:šì</i>	<i>ší</i>	<i>šì</i>	- <i>is</i>
3rd m.	<i>'à:tá</i>	<i>'à</i>	— <i>má</i>	- <i>is</i>

3rd f.	<i>tí:tí</i>	<i>tí</i>	— <i>mú</i>	- <i>it</i>
du. 1st in.	<i>cí:cí</i>	<i>cí</i>	<i>cí</i>	- <i>cin</i>
1st ex.	<i>ní:ní</i>	<i>ní</i>	<i>ní</i>	- <i>nin</i>
pl. 1st in.	<i>cá:cá</i>	<i>cá</i>	<i>cá</i>	- <i>can</i>
1st ex.	<i>ná:ná</i>	<i>ná</i>	<i>ná</i>	- <i>nan</i>
2nd	<i>hú:hú</i>	<i>hú</i>	<i>hú</i>	- <i>u</i>
3rd	<i>sí:sí</i>	<i>sí</i>	— <i>mí</i>	- <i>is</i>

3.6. BOKKOS-*RON* (*cu* 'to eat')

	Aorist: Jussive	Habitual	Progressive	Poss.
sg. 1st	<i>'í:'í cu</i>	<i>'í cwááy</i>	<i>'í má cwayí</i>	- <i>in, -un</i>
2nd m.	<i>há:há</i>	<i>há</i>	<i>há</i>	- <i>a</i>
2nd f.	<i>ší:ší</i>	<i>ší</i>	<i>ší</i>	- <i>iš, -uš</i>
3rd m.	<i>(ʾú):'u</i>	—	—	- <i>is, -us</i>
3rd f.	<i>tí:tí</i>	<i>tí</i>	<i>tí</i>	- <i>it, -ut</i>
du. 1st	<i>kú:kú cu</i>	<i>kú cwááy</i>	<i>kú má cwayí</i>	- <i>ukun</i>
pl. 1st	<i>ká:ká</i>	<i>ká</i>	<i>ká</i>	- <i>ukan</i>
1st ex.	<i>ní:ní</i>	<i>ní</i>	<i>ní</i>	- <i>inin</i>
2nd	<i>hú:hú</i>	<i>hú</i>	<i>hú</i>	- <i>u</i>
3rd	<i>sí:sí</i>	<i>sí</i>	<i>sí</i>	- <i>is, -us</i>

3.7. SHA-*RON* (*ci* 'to eat')

	Aorist: Jussive	Habitual	Progressive	Poss.
sg. 1st	<i>ní:ní ci</i>	<i>ní cáyáy</i>	<i>yín má cíi</i>	- <i>an</i>
2nd m.	<i>yí:yí</i>	<i>yí</i>	<i>ya má</i>	- <i>ah</i>
2nd f.	<i>cí:cí</i>	<i>cí</i>	<i>yic mú</i>	- <i>ac</i>
3rd m.	<i>'á:tí</i>	<i>'á</i>	<i>(tí)má</i>	- <i>as</i>
3rd f.	<i>'á:ti</i>	<i>'á</i>	<i>(tí)mú</i>	- <i>at</i>
du. 1st	<i>gí:gí</i>	<i>gí</i>	<i>gán mí</i>	- <i>gən</i>
pl. 1st in.	<i>gyá:gyá</i>	<i>gyá</i>	<i>gyán mí</i>	- <i>gyan</i>
1st ex.	<i>ní:ní</i>	<i>ní</i>	<i>níí mí</i>	- <i>ní</i>
2nd	<i>ká:ká</i>	<i>ká</i>	<i>kán mí</i>	- <i>ak</i>
3rd	<i>'á:sí</i>	<i>'á</i>	— <i>mí</i>	- <i>si</i>

3.8. KULERE-*RON* (*ci* 'to eat')

	Aorist-Jussive	Habitual(-Progressive)	Poss.
sg. 1st	<i>ní ci</i> <sup>6</sup>	<i>ní cááy(-é)</i>	- <i>in</i>
2nd m.	<i>yá</i>	<i>yá</i>	- <i>yah</i>
2nd f.	<i>kí</i>	<i>kí</i>	- <i>iky</i>
3rd m.	<i>ší</i>	<i>ší</i>	- <i>iš</i>
3rd f.	<i>tí</i>	<i>tí</i>	- <i>it</i>
du. 1st	<i>gí ci</i>	<i>gí cááy(-é)</i>	- <i>igín</i>
pl. 1st in.	<i>gyá</i>	<i>gyá</i>	- <i>igyán</i>
1st ex.	<i>ní</i>	<i>ní</i>	- <i>nih</i>
2nd	<i>kú</i>	<i>kú</i>	- <i>uk</i>
3rd	<i>sí</i>	<i>sí</i>	- <i>is</i>

<sup>6</sup> The aorist and jussive forms in Kulere are identical except for the 1st person sg. which is *nák* in the jussive.

3.9. DERA (KANAKURU) (*tu-* 'to eat')

	Aorist: Jussive	Rel. Past 'Present'	Progressive	Poss.
sg. 1st	<i>na twí</i> (-no) <sup>7</sup>	<i>ndá twa</i> <sup>8</sup>	<i>naa tú-mái</i>	-nó
2nd m.	<i>ka</i> (-ko)	<i>kái</i>	<i>kai</i>	-wó
2nd f.	<i>ši</i> (-ši)	<i>šiji</i>	<i>šiji</i>	-jí
3rd m.	<i>'a:bəla</i> (-ni)	<i>šii</i>	<i>šii</i>	-ní
3rd f.	<i>'a:bəla</i> (-to)	<i>šéé</i>	<i>šee</i>	-ró
pl. 1st	<i>mu</i> (-mu)	<i>mún</i>	<i>mun</i>	-mú
2nd	<i>ku</i> (-ku)	<i>káá</i>	<i>kaa</i>	-mái
3rd	<i>wu</i> (-wu)	<i>wún</i>	<i>wun</i>	-wú

3.10. TANGALE (*saa* 'to eat')

	Aorist-Jussive	Intent.	Habit.	Progressive	Poss.
sg. 1st	<i>'n saa</i> (-nò) <sup>7</sup>	<i>na saa</i>	<i>na gán saa</i>	<i>naŋ saani</i>	-nó
2nd m.	<i>ka</i> (-kò)	<i>ká</i>	<i>ká</i>	<i>kaŋ</i>	-gó
2nd f.	<i>sí</i> (-sí)	<i>sí</i>	<i>sí</i>	<i>siŋ</i>	-jí
3rd m.	<i>béégám</i> (-ní)	<i>béégám</i>		<i>'aŋ</i>	-ní
3rd f.	<i>bástám</i> (-tò)	<i>bástám</i>		<i>'aŋ</i>	-dó
pl. 1st	<i>min</i> (-mù)	<i>mini</i>	<i>mini</i>	<i>minŋ</i>	-mú
2nd	<i>ma</i> (-kù)	<i>máá</i>	<i>máá</i>	<i>máŋ</i>	-gú
3rd	<i>bíígám</i> (-wù)	<i>'anaŋeena</i>		<i>'aŋ</i>	-wú

3.11. PERO (*ci/cwaani* 'to eat')

	Aorist	Habitual	Progressive	Poss.
sg. 1st	<i>né cí</i>	<i>ní gáŋ cí</i>	<i>ní kó tá cwááni</i>	-nò
2nd m.	<i>ké</i>	<i>ké</i>	<i>kí</i>	-yò
2nd f.	<i>ši</i>	<i>shí</i>	<i>ší</i>	-jí
3rd m.	— <i>cí nŋ cáká</i>	— ..	<i>'í</i> ..	-ní
3rd f.	— <i>cí nŋ tè</i>	— ..	<i>'í</i> ..	-rò
pl. 1st	<i>míni</i>	<i>míni</i>	<i>míni</i>	-mú
2nd	<i>má</i>	<i>má</i>	<i>má</i>	-má
3rd	— <i>nŋ šínò</i>	— ..	<i>'í</i> ..	-jù

Note that medio-intransitive constructions are suffixed, e.g. *kúd-éé-ní* 'he refused'.

3.12. NGAMO (*tu-/tii-* 'to eat')

	Aorist	Future	Habitual	Progressive	Poss.
sg. 1st	<i>nè tù-</i> <sup>9</sup>	<i>nè gónnó tiišhé</i>	<i>nè tii-shé</i>	<i>nè zúk tiišhé</i>	-nò
2nd m.	<i>kò tù-</i>	<i>kò gótkó</i>	<i>kò</i>	<i>kò</i>	-kò
2nd f.	<i>ši tù-</i>	<i>ši góóší</i>	<i>ši</i>	<i>ši</i>	-cí

<sup>7</sup> Intransitive-medial verbs take these subject suffixes in addition to the subject prefixes.

<sup>8</sup> *naa twí* etc., is a special habitual form.

<sup>9</sup> This is the base of the aorist-perfect tense; e.g. *nè tùkò* 'I have eaten', *mù tànkò* 'we have eaten'.



3rd m.	— tù-	'à gónní	'à	'à	-ní
3rd f.	— tù-	'à góótó	té 'à	tè	-tò
pl. 1st	mù tàn- <sup>9</sup>	mù gómmú	mù	mù	-mù
2nd	kù tàn-	kù gótkú	kù	kù	-kù
3rd	— tàn-	'à góósú	'à	zùní	-sù

3.13. KAREKARE (*tu-/tee-* 'to eat')

	Aorist	Habitual	Progressive	Poss.
sg. 1st	<i>na tú-<sup>10</sup></i>	<i>na tee-káu</i>	<i>na-a tú-nà<sup>11</sup></i>	-hnow
2nd m.	<i>ká</i>	<i>ká</i>	<i>ká-a</i>	-tukau
2nd f.	<i>cí</i>	<i>cí</i>	<i>cá-a</i>	-ci
3rd m.	<i>sa</i>	<i>sa</i>	<i>sa-a</i>	-hni
3rd f.	<i>ta</i>	<i>ta</i>	<i>ta-a</i>	-tau
pl. 1st	<i>mú tán-<sup>10</sup></i>	<i>mú</i>	<i>má-a</i>	-tum
2nd	<i>kú</i>	<i>kú</i>	<i>kwa</i>	-tuku
3rd	<i>sú</i>	<i>sú</i>	<i>sáa</i>	-sù

3.14. MUBI (after Lukas 1937) (*tii/tuwa* 'to eat')

	Aorist	Present (Habitual)	Preterite (suffixed)	Poss. a) b)
sg. 1st	<i>ní tii</i>	<i>ní túwà</i>	<i>téé-nà</i>	-í -jòò
2nd m.	<i>ká tii</i>	<i>ká</i>	-gá	-dá -dà
2nd f.	? <i>tii</i>	<i>kí</i>	-gé	ʒjíl -jìgè
3rd m.	— <i>tii</i>	—	-gú	-at -dì
3rd f.	— <i>tii</i>	—	-gí	ʒjíl -jíl
pl. 1st	<i>áy tii</i>	<i>án</i>	-néé	-jádá -jìné
2nd	<i>káy tii</i>	<i>káy</i>	-gún	-jígún -jìgun
3rd	<i>ke tii</i>	<i>kì</i>	-gó	-jó -jóó

3.15. MARGI (after Hoffmann 1963) (*wì* 'to run')

	Aorist <sup>12</sup>	Subjunctive	Present <sup>13</sup>	Progressive	Poss.
sg. 1st		<i>kà-y-á-wì</i>	<i>ní á-wì</i>	<i>ní ávàr wì<sup>14</sup></i>	-dyù
2nd m.		-g-	<i>nàg-</i>	<i>nàg-</i>	-ágù

<sup>10</sup> This is the base of the aorist-perfect tense; e.g. *na túkáu* 'I have eaten', *mú tánkáu* 'we have eaten'. Cf. the equivalent forms in Tangale; e.g. *'n saago* 'I have eaten', Bolanci *'n tífwò-*, Ngamo *nè tükò*.

<sup>11</sup> The future tense uses the same base; e.g. *na lá tú-nà* 'I shall/will eat' (cf. the future tense constructions in Margi by means of the auxiliary *ra*, in Wurkun by means of *re*).

<sup>12</sup> Hoffmann (1963), § 273: "The aorist consists of the mere verb stem, i.e. does not show any formative elements apart from the subject expression ... . The subject pronouns may precede or follow the verb". E.g. *lèma gá rá ?* 'where did you go?' (*lèma* 'to go where?').

<sup>13</sup> According to Hoffmann (1963), § 316: "The present has a wide range of meanings, and its name therefore must be considered provisional. It may express: 1. an action in the present, 2. general statements which are not confined to a definite time, 3. possibility or ability, 4. an action in the past,

2nd f.				
3rd m.	-j-	nāj-	nāj-	-ájà
3rd f.				
du. 1st in.	-m-	nàm-	nàm-	-ámà
pl. 1st in.	-mər-	nàmər-	nàmər-	-ámər
1st ex.	-'y-	nà'y-	nà'y-	-á'yà
2nd	-ny-	nàny-	nàny-	-ányl
3rd	-nd-	nànd-	nànd-	-ándà

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5. an action in the future, 6. the subjunctive in certain combinations". It would be interesting to know whether this tense is the one generally used in proverbs. If so it could then well be the equivalent to the Ron present-habitative. Another present form is derived from this by adding *-ra* to the formative vowel *-a-*, e.g. *ni àr-á-wl*. The past tense is derived from the basic present by the suffix *-rì*, thus we have *ni á-wì-rì*.

<sup>14</sup> This is an analytical transcription which does not take into account any juncture features. The subject pronouns can also be suffixed in this tense, e.g. *ávər wì yú* 'I am running'.

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#### DISCUSSION

SKINNER: Again I have questions rather than comments. I would be interested to have Dr. Jungraithmayr's comment on the case of the plural pronoun of the aorist in Hausa, which has a *k*-suffix. I would like to know how he explains that. Then I would also be interested to hear whether in many other Chadic languages there is an indefinite pronoun similar to the Hausa *an*, which in some ways is anomalous in that it adds an *n* in the perfect (completed) instead of becoming a long *a* such as *ya* and *ta* and *na*. The indefinite pronoun corresponding to the French *on* adds an *n* as if it were behaving like a plural, *su* : *sun*, or indeed a feminine, *ki* : *kin*. I would be interested to hear whether this indefinite pronoun also occurs in any of them. And my third point, which is perhaps more of a comment than a question, deals with the possessive suffix for the first person. Newman has suggested elsewhere that this should possibly be analyzed not as *na* but as the genitive link *n* about which we have just been speaking plus *a* as the marker for first person singular. And then of course once again we do have the distinction of gender in this case, where you get *ta* after a feminine word, and Newman has analyzed this into *t* feminine marker plus *a* for first person singular marker.

CARNOCHAN: Could I just make a remark about this last point regarding the suffixes *na* and *ta*. These are, as far as I know, a unique pair in Hausa in so far as, where they are added to words which end with a short vowel, the link is in all cases nevertheless by means of a long vowel. The *na* and the *ta* both end with a glottal stop when they are pausal but they too, when they are linked closely with a following *ba* or something of that sort within the piece, they too—and this again is a unique feature—have a long vowel in this position. So that there is a very peculiar relationship here. It is almost indeed as though one could set these up as *əta* or *əna* in that you have a bit of a syllable as it were as a forming link to these in this relationship. And may I say just one word on the *an* element: In two languages which are geographically close but not related we have this impersonal form with an *a*. In Yoruba, for instance, in *a wá* 'there was a coming' or 'somebody came' it coincides with one form of the 'we' pronoun in the monosyllabic form, but corresponding to 'we' in the more emphatic tense there is a form *awa* but there is no other form for the impersonal. And this also happens in Ibo where you have *ε/a*. This is probably purely accidental, but in any case these three languages all seem to have chosen an open vowel for this impersonal form.

JUNGRAITHMAYR: As regards the impersonal or indetermined pronoun, I cannot think of reflexes in any of the Chadic languages which I know that could be equated with the Hausa *an*, *a*, *aka*, etc. But, since there are some persons here like our colleagues from Hamburg who do have first-hand knowledge of so-called Biu-Mandara or Chadic languages in the narrower sense, perhaps they could comment on that. I personally, as far as the languages of the Plateau are concerned—Angas, Sura on the one hand and Ron on the other hand and the Dera-Tangale group—cannot think of any instance that could serve to support this Hausa *a*, *an*. As to the question raised by Mr. Skinner, on which our Chairman has already commented, that of *na*, *ta*, I am in full agreement and there is nothing really to be added to what Professor Skinner has said. If I did not go into this question it was simply because I felt that it did not have any relevance for what I wanted to discuss here. Perhaps, in the long run, I may be proved wrong. But at the moment I could not see the relevance. Finally, regarding how the *ka* element might be explained from a comparative point of view, the only language I have found so far is Fyer, one of the Ron languages of the Plateau, in which you have a kind of narrative or past, possibly comparable to the Hausa form that is in question, where you also add a *ka* element—but throughout the conjugational system and not only in certain persons—but this is really the only language which I have come across so far where this *ka* element has a similar meaning and possibly a similar, or perhaps comparable, origin.

WOLFF: I am sorry that I cannot supply any indefinite pronoun ending in *-n*, although there is one Chadic language in the north-east of Nigeria that has an indefinite pronoun form which is strange in regard to its vowel. Usually you would expect *a* as the vowel of a pronoun but here we have *o*, and this *o* seems to me to be the same *o* that we have in the pausal form of the noun, or even of the verbal noun, when not in context. In context the ending is *-a*, in *pausa* it is *-o*. As regards the *ka* form in the aorist to which Mr. Skinner was referring, the same as in Fyer, there is a narrative form which may be comparable in the language that I am working on where you have a particle in the shape of *gu*, although it is perhaps not really a particle since it takes the subject suffix directly and has the verbal noun following and not, as it should have, the aspect prefix, the verbal stem, and the subject prefix.

PARSONS: So many points have been raised, may I deal with the last one first. We Hausaists must do more home-work. There are people working in Niger who have written a lot on all this, and you do not have to go outside Hausa. I cannot tell you which dialect, but there is certainly one which has the full set: *na-ka*, *ka-ka*, *ki-ka*, and so on. Wherever it has come from, standard Hausa has lost it in this same group of persons which has lost the nasal. I must just mention, as they are neither of them here unfortunately, that Peter Zima of Prague and Claude Gouffé of Paris have done a great deal of original research, and have pretty well covered the Niger Republic bringing to light very many interesting things for Hausa at all

levels. But of course the tense system of Hausa is where there is most dialectal difference, even in the number and function and so on of the tenses. We are too apt always to quote Kano for Hausa forms. Take for instance the aorist in Sokoto. You get various things: *niz zo* with gemination, *kaz zo*, *kiyka zo*, *taz zo* and so on; even within Nigeria there are many forms. And I just want to say on that score that, however much I may feel that there are affinities to Ancient Egyptian and so on, I am quite happy that all the conjugational system of the verb in present-day Hausa is pan-Chadic, whether it is borrowed or is the result of some culture-contact. Though I would rather wonder whether there was ever a stage in which the verb was conjugated by means of suffix pronouns, which after all quite clearly could be added to the grade endings and then have dropped off. And in that context, it is worth just commenting on the single anomalous verb in Hausa, which is incidentally tenseless too, *za*, the basis of *zo*, 'come' and *je*, and so on. It means 'to go'. It is a present tense with a vague future meaning. And in that case the pronouns including the fourth person come after the verb—*ina zaa a* is a perfectly good Hausa sentence meaning 'where is one to go?', 'there is nowhere one can go'. And that brings me to the question of *a*. I do not know, but I will just throw out another possibility. I prefer rather to call it a fourth person than an impersonal for the reason which I have only recently established: that it must have a personal referent, human or God; it can never be used for animals. And finally, if I may just refer to Mr. Carnochan, I would like to extend what he said regarding the question of the *a* and the peculiarities of the first person genitive. Of course when you get this *din* form I suppose you will have to have *a* twice over because the equivalent first personal form, e.g. 'my cup', is *kwaf dina* with a long *i*, whereas in the other forms it is a mere syllabic nasal. And furthermore it has a long vowel, you have established, I think, unlike all the other endings, in all contexts except pause. Abraham is wrong over that. Otherwise the pronouns are identical with, for example, the dative suffixed pronouns, but the dative suffix first person is either *mani* or *mini*, always something with a nasal. This *-āwā* is quite unique.

MARCEL COHEN: It seems to me that I cannot neglect to participate to some degree in this discussion. I very much regret that Diakonoff, who was the first to include Chadic in a manual of Hamito-Semitic, is not present amongst us. I also regret the absence of Gouffé, who is our specialist on Hausa matters in France and who knows the material well and has examined it from the comparative point of view. And I regret even more that I am only a part of my real self and that I am very poorly up to date with things. I only know imperfectly the work of Greenberg and of Diakonoff, I have not been able to read the papers presented at this Colloquium and my knowledge of English is not always sufficient for me to be able to follow all the speakers. So I start by asking you to excuse my ignorance and my failings. But this said, I would like to go on to say that we are only on the threshold of the problem under discussion and that this cannot be considered settled one way or the other. My teacher Meillet

used to say that it is impossible to prove that two languages are NOT related to one another and I believe that to be true. It is always possible for there to be accidents in the history of a language. I referred earlier to the irregularities which separate Romance from Latin and which set Egyptian quite apart in the Hamito-Semitic domain. So one must clearly admit that nothing is impossible and that things which appear extraordinary can be integrated into an evolution which really took place. This has its importance for the matter which here concerns us, namely the problem of what is the exact position of the Chadic languages. I think that there is still a lot of comparative work to be done in various sectors of the language. Earlier we were discussing the question of Hausa possessing certain phonemes peculiar to it, phonemes which were missed by the earlier Hausaists who failed to hear them. Are there other African languages which have these same sounds? They are not Semitic and they are not Berber. It is a question that will have to be examined. And what can be said regarding the morphology, which is certainly the most important area of a language. We find that the Hausa conjugation operates by means of pronouns and that these pronouns resemble very closely those of Hamito-Semitic. This raises several problems. Firstly, is it possible for a whole set of pronouns to be borrowed? Could we have there an areal feature—a case of *affinité*, of adstrate, of loaning rather than one of genealogical relationship? Well, clearly it is rare for a whole morphological set to be borrowed but it is not unknown, or at least partially. A part of the English pronouns were borrowed from Danish—there is a mixing in the English pronominal system of Anglo-Saxon and of Danish, although obviously both are ultimately of Germanic origin. So there is, I think, a precedent there and one could find others. Secondly, one must ask what is the way in which these pronouns function. But the functioning of the Hausa verb is not the same as in Arabic or Berber. The pronouns are not attached to the verb but can be separated from it by the insertion of certain particles. If we turn to the rest of the verb we may ask “Does Hausa have the same fundamental opposition that we find in Hamito-Semitic between an *accompli* and an *inaccompli*?”. I think “yes”. But then Delafosse claimed the same opposition for all the languages that he called Negro-African. Of course it is quite possible for two neighbouring groups which are not related to share a feature of this type. We must compare Hausa not only with Hamito-Semitic but with the other Negro-African languages. In *Les Langues du Monde* I classed Hausa among the Negro-African languages; I agree that this requires rectification, but we must also say why. We must demonstrate that Delafosse, who was the author of this classification, was mistaken. Then there is also the question of gender. As you are aware Meinhof classed a quantity of African languages together on the grounds that they have gender, masculine and feminine, and on this basis in particular he grouped Fula with the Hamito-Semitic languages. There remains the question of vocabulary. Vocabulary is important and that is why I spent twenty five years of my life preparing my *Essai Comparatif* in order to see whether, in addition to the morphological similarities which I might call blindingly obvious between the four groups recognized as Hamito-Semitic, there

might also be a stock of common vocabulary. It turned out that it does exist but in much smaller measure than in Indo-European, or in Finno-Ugrian or the Bantu languages. And so, in my *Essai*, Hausa occupies a relatively modest place in so far as lexical correspondences are concerned. I did find some, but they were relatively few. Since then, however, work has continued and many more correspondences of vocabulary have been recognized. That is important. Gouffé admits that there are many more valid lexical comparisons than I gave, but at the same time he rejects many of those that have been proposed by others. So, finally, what I would call for is more comparative studies in all these sectors of the language before we can really decide to put the Chadic languages into a single group with the four branches of Hamito-Semitic which are clearly related to one another.





## THE STRUCTURAL STATUS OF HAUSA *R* SOUNDS\*

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In most accounts of Hausa phonology, two *r*-like phonemes are set up: *ɾ* and *r*.<sup>1</sup> The first has usually been described as a flap, the second as a trill (Greenberg 1941). According to Ladefoged (1968, 30), *r* "is a trill which has a statistical probability of consisting of only one tap"; *ɾ* "appears to be a retroflex tap". He adds, curiously enough, that he has not been able to find any consistent acoustic difference between the two sounds in the speech of his Hausa informants. Since in this paper we shall discuss only the status of these sounds within the Hausa phonological system, the exact phonetic realization of them is really irrelevant for the rest of our discussion.

In the bi-unique tradition of phonemic analysis, these two phone types constitute separate phonemes, at least for the pronunciation recorded in Bargery's dictionary (1934, XXII f.), but with an overlapping distribution. A number of minimal or near minimal pairs can be found, e.g., *bàràa* 'begging'—*báɾàa* 'a servant', *màràfiyàa* 'a kind of cap'—*màɾàfiyàa* 'putting on airs', *kàráu* 'thoroughly'—*káɾáu* 'glass bangle'. But only *r* occurs word final or before *n*. On the other hand, *r* rather than *ɾ* before a vowel rarely if ever occurs in native Hausa words, and the use of *r* in such a position in a particular word has been used as evidence for considering that item to be a loan-word. This has been true most notably in Greenberg's argument (1960) for the Kanuri origin of certain words in Hausa, e.g., *gàarúu* 'city wall', *káarùwáa* 'prostitute', *ràafànli* 'mother's brother', *rùbùutúu* 'writing', and a few others.

With very few (if any) exceptions, *ɾ* and *r* would be in non-contrastive and complementary distribution if loan-words could be separated from the rest of the vocabulary. In line with the procedures of generative grammarians such as Chomsky and Halle (see, for example Halle 1964a, 1964b; Chomsky and Halle 1968), this could be done by specifying for all words the presence or absence of a feature [Foreign]. In words marked [+ Foreign], for example words of Arabic or English origin, one would find only *r*. In [-Foreign] words, *r* and *ɾ* would be variants of a single unit. Of course,

\* This paper was not discussed.

<sup>1</sup> Transcription used in this paper is somewhat inconsistent for practical reasons. Sometimes the official Nigerian Hausa orthography is used where both sounds are written as *r*.

some words of foreign origin may have been so completely assimilated into the language that they too would be marked [-Foreign].

The purpose of this paper is merely to point out that the facts are much more varied than Bargery's treatment and that of other observers suggest. We shall here present a summary of a very small survey of twenty native speakers of Hausa from various parts of the Hausa language community.<sup>2</sup> All informants were questioned with regard to whether they made a phonemic distinction between the two *r*'s or not and then were asked their pronunciation of 102 loan-words, which were grouped into the following categories: (1) old Arabic loan-words [16 items], (2) new Arabic loan-words [30]—the distinction between the two was established by using the criteria given by Greenberg (1947) and Gregersen (1967a, 179-183; 1967b); (3) Islamic names and pet names derived from them [20]; (4) loan-words from Kanuri and other non-Arabic sources [8]; (5) English loan-words [28].

Four general patterns emerged:

(1) The system described by Bargery, with two phonemes in essentially the distribution suggested by him.

Nine informants fell into this group.

(2) One phoneme, usually with *ɾ* and *r* allophones distributed as described above. Four informants fell into this category and only one speaker departed from the allophonic pattern; he was from Ghana. He was recorded as using a flap *r* before *i* and *e*, and a trill *r* elsewhere initially.

(3) Style variation. One informant from Zaria was found to answer the questionnaire with a pattern identical to (1), viz. two phonemes in Bargery's distribution, but the same informant in informal conversation dropped the distinction and used *ɾ* and *r* as allophones of a single phoneme. (Another informant from Lagos, here classified as belonging to (2), himself suggested that in certain styles he might vary and even in native Hausa words found an interchange of *r* with *ɾ* quite acceptable and natural; e.g., the word *ruwa* 'water' was normally pronounced with *ɾ*, but could just as well be said with *r*. This informant was also a native speaker of Yoruba, but had spent several years in northern Nigeria).

(4) Two phonemes, but with considerable divergence from the Bargery statement on distribution. Five informants showed this pattern. Most of these informants showed a fairly haphazard pattern with all the categories of word included, although Kanuri loan-words were normally all pronounced with *r* (two informants were exceptions here). An 18 year old Hausa Christian woman, born in Lokoja, brought

<sup>2</sup> We are indebted to F. W. Parsons and J. C. H. Farmer for introductions to several of the informants used.

up in Zaria and educated in a mission school for her primary education, showed a considerable deviation for all categories but English loan-words, all of which she pronounced with *r*. Interestingly enough, of the total sample of people with two phonemes, at least one English word on the questionnaire *garmaho* 'gramophone, phonograph' was pronounced with *r* by eight informants although the Christian woman used *r*.

The fact of style variation partially supported by the usage of the Christian informant suggests that a tradition of correctness has been superimposed upon the speech of many people and that there may be some correlation between the use of *r* and traditional religious school training. Although the present study does not investigate the problem further and does not produce any meaningful isoglosses, a more thorough investigation would have to take into account not merely geographical but other sociolinguistic (particularly religious) factors as determinants of usage on this point.

As for historical reconstruction, nothing definite can be said as to when the originally allophonic relationship between *r* and *ɾ* was phonemicized. The word written *labari* 'news, story' (Arabic *al-xabar*) in the official orthography may supply a clue, however. The 'puristic' pronunciation listed by Bargery and used by several informants has *r*, but many speakers use *ɾ* instead. Indeed, Kraft, who normally distinguishes between the two in his pedagogical grammar and (less frequently) in his reader of Hausa, indicates only a pronunciation with *ɾ* (Kraft and Abubakar 1965). As we have tried to indicate elsewhere, the distinction Greenberg has made between old Arabic loan-words (I) and newer ones (II) may be subdivided still further so that the older loans may be classified into an earlier (Ia) and a later stage (Ib). None of the Arabic loan-words with *r* can unambiguously be grouped with the earlier old loans (Ia), but it seems plausible that at one stage foreign *r*-sounds before vowels were simply borrowed as *ɾ* and that only later on a tradition was established of using *r*.<sup>3</sup> If this is true, then *làabáárìi* would probably be a Ia word, and the *làabáarìi* variant a reborrowing at a later date. But, needless to say, the facts are not clear.

#### APPENDIX

The questionnaire included the following items:

##### I. Old Arabic loan-words

*Alkur'an* 'Koran', *allura* 'needle', *araha* 'cheap', *ashirin* 'twenty', *aradu* 'thunder', *alkarya* 'main town in district', *dara'a* 'cubit', *fakiri* 'poor person', *harsashi* 'bullet',

<sup>3</sup> Some speakers apparently tend not to use *r* before *i*, only *ɾ*, so that the matter is even less clear than suggested in the present paper.

*kadari* 'value', *kirfa* 'cinnamon', *lahira* 'next world', *labari* 'news', *laraba* 'Wednesday', *riba* 'profit', *warka* 'page of book'.

Several of these words were not used at all. For example, the most common from for 'Koran' was *Alkur'an* with *k* rather than *ḳ*. Other words not commonly used were *dara'a fakiri*, *kirfa*.

The usage of informants showing pattern (4) was as follows:

The Christian woman informant used *ṛ* in *ashirin*, *aradu*, *labari*, *riba*.

A 25 year old man born in Ilo, brought up in Bankanu, with a Koranic school education used *ṛ* in quite a few items: *araha*, *ashirin*, *dara'a*, *fakiri*, *harsashi*, *kadari*, *kirfa*, *labari*, *riba*.

A male informant, 27 years old, born in Tessaoua in Niger, brought up in Niamey, used *ṛ* in the following words: *ashirin*, *fakiri*, *labari*, and most interestingly in *lahira*, a religious term. This informant had not had a Koranic school education and had not studied Arabic although he was Muslim.

A male informant born in Tambuwal in Sokkoto province and brought up in Kano, about 35 years old with a Koranic school education, used *ṛ* in the following words: *allura*, *aradu*, *labari*, *riba*.

A male informant from Kano, 30 years old with a Koranic school education, used *ṛ* perhaps most frequently of all. He used *ṛ* in *allura*, *araha*, *dara'a*, *kadari*, *lahira*, *labari*, *laraba*.

Other informants used either *r* or *ṛ* consistently in these words.

## II. New Arabic loan-words

*al'amari* 'business, matter', *al'aura* 'pudendum', *albaras* 'leprosy', *albarka* 'blessing', *alharini* 'silk', *alheri* 'kindness', *alkawari* 'promise', *Alkur'an* 'Koran', *almajiri* 'pupil', *asiri* 'secret', *attajiri* 'merchant', *barka* 'greetings', *barradi* 'teapot', *daraja* 'rank, honour', *gadariyya* 'pistol', *gaira* 'deficit', *haraji* 'land tax', *haram* 'forbidden', *ijara* 'commission', *ishara* 'indication', *jarraba* 'test', *mubazzari* 'extravagant person', *muradi* 'wish, desire', *ruku'u* 'bending posture in prayer', *shari'a* 'law', *shawara* 'council', *shukura* 'gratefulness', *taraha* 'mattress', *waziri* 'vizir', *za'afaran* 'saffron'.

A number of these words were not commonly used, e.g., *barradi*, *gadariyya*, *ijara*, *muradi*, *taraha*, *za'afaran*.

The usage of informants showing pattern (4) was as follows:

The Christian woman informant used *ṛ* in *alkawari*, *almajiri*, *asiri*, *attajiri*, *ijara*, *ishara*, *mubazzari*, *shari'a*, *shukura*, *waziri*.

The man from Ilo used *ṛ* in far many more words: *al'amari*, *al'aura*, *alharini*, *almajiri*, *asiri*, *attajiri*, *daraja*, *haraji*, *haram*, *mubazzari*, *muradi*, *ruku'u*, *shawara*, *taraha*, *waziri*. (Note especially the use of *ṛ* in the religious words *haram* and *ruku'u*).

The man from Niger used *ṛ* in *al'amari*, *alheri*, *almajiri*, *asiri*, *attajiri*, *jarraba*, *mubazzari*, *muradi*, *shari'a*, *waziri*.

The man from Tambuwal used *ɾ* in *al'amari* (but *r* in the plural *al'amura*), *alheri*, *attajiri*, *barradi*, *gaira*, *jarraba*, *shawara*, *shukura*, *taraha*.

The man from Kano used *ɾ* more frequently than *r*. He used *ɾ* in *al'amari*, *al'aura*, *alheri*, *alkawari*, *almajiri*, *asiri*, *attajiri*, *daraja* (also with *r*), *gaira*, *haraji*, *ijara*, *ishara*, *muradi*, *shari'a*, *shawara*, *shukura*, *taraha*, *waziri*.

### III. Islamic names, etc.

*Ibrahimu*, *Ibiro*, *Ibiru*; *Haruna*, *Haro*, *Haru*; *Maryama*, *Mairo*; *Sarati*, *Sarai*; *Zakariya*, *Zakari*; *Bature* 'European', *Bature* (boy's name), *Turai* 'Europe', *Turai* (girl's name); *Balarabe* 'Arab', *Balarabe* (boy's name), *Nari*, *Narai*.

*Saratu*, *Lari*, *Larai* were common variants for *Sarati*, *Nari*, and *Narai*, respectively.

The Christian woman used *ɾ* in *Haru*, *Bature* (both meanings), *Turai* (both meanings).

The man from Ilo used *ɾ* in most of the items: *Ibiro*, *Ibiru*, *Haruna*, *Haro*, *Haru*, *Maryama*, *Zakariya*, *Zakari*, *Bature* (both meanings), *Turai* (both meanings).

The man from Niger used *ɾ* only in *Mairo*.

The man from Tambuwal did not use *ɾ* in any of the forms.

The man from Kano used *ɾ* in *Ibrahimu*, *Sarati*, *Zakariya*, *Bature* (boy's name, but not 'Europe'), *Balarabe* ('Arab', but not boy's name), *Lari* (for *Nari*).

Some other informants used *ɾ* in *Ibiro*, less commonly *Ibiru*, but not elsewhere.

### IV. Kanuri and other (non-Arabic, non-English loan-words)

*garu* 'city wall', *goro* 'kola nut', *karanta* 'read', *karatu* 'reading', *karuwa* 'prostitute', *rubuta* 'write', *rubutu* 'writing', *soro* 'flat-roofed house', *rafani* 'mother's brother'. (The last item was inadvertently omitted from some schedules).

The Christian woman used *ɾ* in *garu*, *goro*, *karuwa*, *rubuta*, *rubutu*, *soro*.

The man from Ilo used *ɾ* in all items except *karuwa* and *karatu*.

The other informants showing pattern (4) used *r* in all the words.

Nearly all the informants showing pattern (1) used *r* exclusively in these words. One informant, a carpenter 27 years old from Zaria, used *ɾ* in *garu*, but not elsewhere.

### V. English loan-words

*adireshi* 'address', *darekta* 'director', *direba* 'driver', *fensiri* 'pencil', *fensirori* 'pencils', *firamare* 'primary school', *firimiya* 'premier', *gareji* 'garage', *garmaho* 'gramophone, phonograph', *janareto* 'generator', *Kirista* 'Christian', *kyamara* 'camera', *laburare* 'library', *lebura* 'laborer', *libarba* 'revolver', *majistare* 'magistrate', *orinji* 'orange', *rahoto* 'report', *rasit* 'receipt', *razdan* 'resident, provincial advisor', *rehul* 'raffle', *reliwe* 'railway corporation', *rula* 'ruler', *sakandare* 'secondary school', *sukuddireba* 'screwdriver', *tarho* 'telephone', *teburi* 'table', *teburori* 'tables'.

The Christian woman used only *r* in these words.

The man from Ilo used *ɾ* in most words: *adireshi*, *direba*, *gareji*, *janareto*, *laburare*, *lebura*, *rahoto*, *rasit*, *razdan*, *rehul*, *reliwe*, *rula*, *sakandare*, *tarho*, *teburi*, *teburori*.

The man from Niger used few English loan-words to begin with, often employing French terms instead, e.g. *lakol* (l'école) for *firamore*. He said *talho* for *tarho*. The word *gareji* was pronounced with *ɾ*, as was *oranji* (rather than *orinji*).

The man from Tambuwal used *ɾ* only in *majistare* and *sakondare*.

The man from Kano used *ɾ* in *fensiri* (but not in plural *fensirori*), *firimiya*, *kyamara*, *lebura*, *rahoto*, *rasit*, *teburi* (but not in plural *teburori*). (It is unfortunately not clear from our notes whether both *r*'s in the plural forms are pronounced the same way, although presumably they are).

The informants showing pattern (1) used *r* in all these forms except frequently for *garmaho*, and less commonly *orinji*.

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## HAUSA AND CHADIC

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### I. INTRODUCTION

The following is a rewritten and considerably modified version of my paper entitled "Prolegomena on the Status of Hausa" which was presented at the Colloquium. This former paper was very hastily written and somewhat ill-constructed and diffuse, and some of the statements it contained were—I may say deliberately—provocative, though they failed to provoke much discussion at the time. Furthermore it suffered from the handicap that my article "Is Hausa really a Chadic language? Some problems of comparative phonology" (Parsons 1970) had not yet appeared in print, and I was unable to make any advance reference to it, since it formed part of a then unadvertised 'Festschrift'.

I may now, however, refer to this article and quote two sentences therefrom. "My arguments (sc. against the inclusion of Hausa as a Chadic language) fall under three heads: (1) phonology, (2) morphology and (3) vocabulary or lexicon ... . In this present article I shall be dealing only with the phonological arguments, leaving the morphological and lexical arguments, together with my general conclusions, to follow later". This present paper attempts to set out some of the morphological and lexical arguments. It does NOT repeat the very tentative and speculative theories as to the origins and status of Hausa that were put forward in the paper presented at the Colloquium. With so much research now in progress on Chadic languages, notably by Jungraithmayr and by Newman and Ma, I now feel that these theories may have been over-hasty and premature, and I would prefer in this paper to confine myself mainly to INTERNAL evidence regarding the Hausa language. (I make no apology for the volume of Hausa words cited in this article, which follows the Greenbergian principle of mass comparisons, i.e. the more the examples, the stronger the argument).

I must, nevertheless, re-declare my scepticism with regard to many of the postulates of comparative genetic linguistics, especially in the classification of African languages. In particular I am perplexed by the very small number and proportion of items in any one African language for which cognates have been found in any other African

language. Thus, to take the most obviously homogeneous group of languages, Bantu,<sup>1</sup> we have it on the authority of Guthrie that no one Bantu language contains more than 20 per cent of Common Bantu vocabulary. Surely it is a matter of linguistic, as well as historical, concern to seek to know where the other 80 per cent in the 'most Bantu' languages came from? Bantu affinities, however, are based, not simply upon cognate items of vocabulary, but upon wider phonological, morphological and syntactical resemblances, *par excellence* upon the common feature of a concordial class system. But when we turn to the heterogeneous languages of what Dalby has aptly termed the "Sub-Saharan Fragmentation Belt" of Africa (Dalby 1970, 163 sqq.), which embraces Hausa and all the so-called Chadic languages, such common features—other than a two-way gender system and perhaps some common pronominal and other morphemes—are harder to find, and the tendency seems to have been to rely almost entirely upon apparently cognate items of vocabulary.

## II. VOCABULARY

I shall start this discussion, therefore, with the vocabulary of Hausa, which, if not as yet totally recorded in any dictionary, is at least sufficiently well-known and documented for purposes of statistical research. Furthermore enough is now known about the rather complex and combinationally restrictive phonology of the language to enable us—even without collateral evidence—to separate out most of the many loan-words it contains from the purely indigenous elements of vocabulary.<sup>2</sup> In general too it may be said that, whereas loan-words constitute a high proportion of the NOMINAL words in the lexicon, the great majority of VERBAL words<sup>3</sup>—discounting a fairly considerable number of verbs taken from Arabic—are native to the language. And the great majority of these verbs have a basically 'concrete' meaning, i.e. they indicate actions of one sort or another, and the meanings of most of them signify actions which, if not 'universals', at least may be presumed to have been part of everyday Hausa life from a very early time. They may therefore be assumed to constitute one of the oldest, if not the oldest, strata of the vocabulary.

Now there are some striking features about these Hausa verbs which I should like to emphasize, viz.:

(1) Their morphological uniformity. I am not here referring to their flexional system of grades and subsidiary forms, which will be discussed later in this paper,

<sup>1</sup> It is irrelevant to my argument what position one takes in the classificatory controversy initiated by Greenberg, i.e. as to whether the Bantu languages constitute a family, a group, or a sub-group.

<sup>2</sup> This point was elaborated in an unpublished paper on English loanwords in Hausa read to the Philological Society of Great Britain in 1959.

<sup>3</sup> These include, not only verbs proper, but verbal nouns and also many 'dynamic' (i.e. signifying actions) nouns which have no parent or cognate verbal forms extant.



but to the forms of their bases, i.e. the verb shorn of its tone-pattern and terminal vowel. About 70 per cent of these are monosyllabic, comprising—as do all disyllables in the language—either a CVC- or a CVCC-structure.<sup>4</sup> The remaining 30 per cent have a disyllabic or trisyllabic structure, following certain set patterns which need not here be set out in detail, and observing certain prescribed restrictions with regard to both consonant and vowel sequences (see further Parsons 1960, 5-6). It may be added that the question whether these polysyllabic verbs represent extended disyllabic ones, or are simplex, radical verbs, is at present unresolved: there is considerable evidence in both directions.

(2) Their sheer number. On a conservative estimate I would reckon that there are some 1,000 disyllabic and another 1,000 polysyllabic verbs, mostly in common everyday use.<sup>5</sup>

(3) The high proportion of the permissible combinations of phonemes or sounds in the language that is represented in this verbal vocabulary. Thus, taking two of the most common initial consonants, /t/ and /k/, and following these with the commoner of the two vowel phonemes, /a/, and either C or CC, of the 167 and 161, respectively, possible combinations, 37 or 22 per cent, and 26 or 15 per cent, respectively, actually occur as verbal bases.<sup>6</sup>

(4) The fact that a great many verbs may be subdivided into groups of anything from three or four to thirty or more on what may be termed a 'phono-semantic' basis. That is to say that groups of verbs that have one or more consonantal sounds in common, and in the same place in the word, also share a common substratum of meaning. Most commonly the significant sounds are either the C<sub>2</sub> or final consonant of the base, or else a combination of this consonant and a preceding coda consonant, these sounds being, as it were, generic to the group, whilst the C<sub>1</sub> or initial consonant plus the intervening vowel serve to give each member of the group its specific, distinctive meaning. The vowel in most cases is lexically significant, but phono-semantically immaterial, though in a few groups it too forms part of the common phono-semantic denominator. Here are some examples:

(i) The 'pressure' group. 90 per cent of the disyllabic verbs, and also some trisyllabic verbs, whose C<sub>2</sub> (in this group ignoring the coda consonant, if any) is /ts/ signify some form of pressure or protrusion (sometimes with an added notion of

<sup>4</sup> The initial C includes the (standardly unwritten) glottal stop, and the CVCC- structure embraces, not only bases containing a coda consonant, but also those having a long vowel or diphthong. For the phonological arguments see Parsons 1970: 277-278.

<sup>5</sup> Many of the latter have escaped recording in the dictionaries. The latter figure, note, includes a fair number (possibly half) of denominative and de-ideophonic verbs and loan verbs from Arabic.

<sup>6</sup> These include some verbal bases having dialectally variant shapes. The total number of disyllabic verbs with *t*- initial is over 60.

profusion), e.g. *mats-* 'press, squeeze, urge', *maats-* 'compress', with diminutive/applicative extension *-n-*, *muntsun-* 'pinch', *laats-* 'squeeze gently' (e.g. fruit to test ripeness), *loots-* 'cave in under pressure', *luuts-* '(body, teeth) sink into soft substance', *nats-/nits-/nuts-* 'become sunk or immersed' (in water etc.), *raats-* and *feets-* 'cleave way through', *rits-/ruts-* 'hem in', *rints-/runts-* 'grip tightly, screw up (eyes)', *roots-* 'dent, break (round brittle object) by pressure from above', *ruuts-* 'plunge (knife) into', *dats-* 'dam, intercept', *daats-* 'chop, breach', *dirts-* 'press down on with edge or tip', *burts-* 'exude in one place when squeezed in another', *buntsur-* 'purse (lips), joggle (buttocks), stick (tail) in air', *fantsam-* 'splash, scatter', *fartsar-* 'grind coarsely', *gantsar-* 'bend (bow, back) into curve, etc.', *gaats-* 'bite (at/off)', *garts-* 'sink (teeth) into', *kats-* 'snap under strain', *kaats-* 'scrape, plane', *kuuts-* 'force way in', *kwantsam-* 'utter under mental pressure, blurt out; produce abundantly', *taats-* 'express liquid (from), milk' (contrast *taat-* 'filter, strain', where no pressure is involved), *tarts-* 'spurt', *tartsatsii* 'sparks', *toots-* 'thrust, squeeze, push in' (e.g. additional stick into bundle), *tirts(aats)-* 'thrust (e.g. spear) in, stab', *tuntsur-* 'topple over', *tuutsuu,* *buutsarii* '(donkey) bucking', *wantsal-* 'fall headlong, roll on to', *waats-* 'scatter, sow broadcast, throw out', *yantsar-* 'produce (fruit) prolifically', *rants-* 'assume great proportions; asseverate, swear'. Even *moots-*, the general word for 'move', may be said to involve SOME muscular pressure.

(ii) The 'friction' group. These have C<sub>2</sub> /z/, preceded either by a vowel (generally /u/) plus /r/, or by a long vowel sound (generally /ii/ or /uu/), e.g. *barz-* 'grind coarsely', *darz-/tirz-/turz-/tuuz-* 'slide, slither, scrape, squash underfoot, come to slithering stop', *furz-* 'spray (water etc.) from mouth', *garz-* 'eat a lot of crunchy food', *gurz-* 'graze, scrape, gin (cotton)', *jiiz-* 'scrape off', *kurz-/kuuz-* 'scrape, abrade', *murz-* 'rub between palms, massage', *kwarzan-* 'scratch painfully or injuriously' (with the same diminutive/applicative extension as *muntsun-* in group (i)), *raaz-* 'erode', *zooz(ay)-* 'rub, wear away, erode'. (Gentler friction is represented by C<sub>2</sub> /s/, e.g. *laas-* 'lick', *soos-* 'scratch pleasurably', *waas-* 'whet').

(iii) The 'displacement or disalignment' group. These have C<sub>2</sub> /d/, preceded either by a vowel (generally /i/u/) plus /r/, or by a long vowel or diphthong, e.g. *murd-* 'twist, wring, contort', *gurd-* 'twist out of shape, sprain, dislocate', *jird-* 'ditto; uproot (tree)', *fard-/fird-/furd-* 'slit open (e.g. fish), hoe up (e.g. groundnuts)', *hard-* 'interlock, cross (legs etc.)', *baud-* 'swerve, dodge, step aside', *buud-* 'uncover, open', *cuud-* 'knead, handle roughly, clash with', *faad-* 'fall', *feed-* 'flay, draw (fowl)', *huud-* 'bank up earth round crops', *kaad-* 'tease (cotton)', *kardaad-*, *kurd-* 'make detour, zigzag, thread one's way', *kood-* 'whet, roughen (grindstone)', *reed-* 'pare, scrape (meat) off hide', *ruud-* 'mix, confuse, perplex, mislead', *taad-* 'trip', *yaad-* 'skim', *zaud-* 'fidget, edge away'. If the generic meaning is extended somewhat to include application of an arm or implement (as opposed to keeping it still), we may add a whole sub-group of verbs differing only in their C<sub>1</sub> that are used synonymously with such objects as *maarii* 'slap' or *buulaalaa* 'whip', viz. *caud-*, *gaud-*, *kaud-*, *shaud-* and *taud-*; also *fyaad-* and *raad-* which are similarly used (cf. the basic meaning of

'apply' i.e. bend towards). In the case of trisyllabic verbs this basic meaning of displacement is conveyed simply by the  $C_3$  /d/, with any preceding sounds, e.g. *bankad-/hankad-* 'lift edge of, open (door); jostle', *girgid-* 'shake, loosen in socket', *gyangyad-* 'nod (head in drowsiness)', *kalmad-* '(tool) bend, buckle', *kwankwad-* 'knock back (drink, journey)', *murgud-* = *murd-*, *muskud-* 'fidget, edge away', *rambad-* 'apply (whip etc.)', *sumbud-* 'flag', *shaagid-*, *ki-shingid-* 'put askew, recline on the side', *shimfid-* 'spread out (thing normally rolled up, e.g. mat)', *tankad-* 'winnow, knock over, capsize', *targadee* 'a sprain', *tazgad-* 'tilt', *tittid-* 'give climber a push up', *tunkud-* 'push aside', *turbud-* 'thrust into hot ashes, bury unceremoniously', *zaakud-* 'mix up, 'fidget' *zunkud-* hitch up (load, baby on back), (maggot) push up (grains of corn)'.

(iv) The 'squenching or liquidity' group. The objects of all these verbs are either liquids or things containing some moisture. They have the combination either /rf/ or /rb/ in their roots, e.g. *kurɓ-* 'sip', *kirɓ-* 'pound moistened substance', *shaɓɓ-*, *zurɓ-* 'drink noisily, swill; cut unripe corn or green wood', *yarɓ-/yarf-* 'splash with mud, flick with wet cloth', *kwarf-*, *kwarfɓ-* 'dip, bale out small remaining quantity of liquid', *surf-* 'pound (moistened corn to remove bran)', *tarf-* 'pour in drops', *gurɓaat-* 'stir up sediment in (water)', *shirɓun-* 'rub (oil into skin)' (the -n-, again, is applicative).

(v) The 'violent or vehement action' group. These have the combination /rg/ in their roots, e.g. *burg-* 'whisk, swizzle, brag, bluster, intimidate, excite', *harg-* 'wolf (food)', *hargoowaa*, *hargaagii* 'uproar, angry speech', *shirg-* 'do a lot of, pile on', *tsarg-* 'show hostility to, ostracize, persecute', *wurg-* 'hurl, fling, drive or travel fast', *zarg-* 'attach noose to, lasso; voice suspicion of, blame', perhaps also *farg-* 'come to one's senses, become suddenly aware of a situation'. This /rg/ is combined with the /ts/ of group (i) in *hargits-* 'muddle' and *hargitsii* 'confusion, dissension', and with the augmentative /m/ extension in *birgimaa* '(horse, donkey) rolling on ground, being frisky'.

(vi) The 'rapid movement away' group. These have either the combination /sk/ or /zg/ in their roots, or else a  $C_2$  /g/ preceded by the long vowel /ii/ or /uu/, e.g. *fizg-* 'wrench, whisk, snatch (away)', *wazg-/wuzg-*, same meanings, *ɓazg-* 'rip off (branch etc.)', *tizg-/tuzg-* 'wrench, tug (out); slide, slither down', *tuug-* 'uproot', *fiig-* 'pluck (feather, hair, bird), rip, strip (heads of corn), snatch away, whip out (sword), lift (horse) into jump etc.', *tsiig-* 'pull out (hair, burr)' *ruug-* 'rush, race off, drive away', perhaps also 'ask- 'shave', *hask-* 'light (fire, lamp), (sun) shine forth' and *faskar-* 'split (wood)'. The meaning is extended to large scale actions in *kuug-* and *zuug-* used of making big fires, applying in excess etc., and *tsuug-* 'pour a lot of, rain heavily'.<sup>7</sup>

<sup>7</sup> These—and numerous other—'augmentative' verbs have no precise meaning in themselves (other than this general 'deep grammar' one), but take their particular actional application from the meaning of the noun, concrete or dynamic, which stands as their object. Compare, perhaps, such verbs as 'deal, slosh, swipe' in English.

(vii) The 'rotatory or backwards and forwards movement' group. These are mostly trisyllabic verbs of geminate structure, i.e. the root is repeated,<sup>8</sup> which are characterized by the co-occurrence (in all but one verb in combination with another consonant) of the semi-vowels /w/ and /y/, in that order. In standard Hausa the vowel is always /a/ and the /w/ associated with the C<sub>1</sub> in the form of W-coloration has disappeared, leaving only the C<sub>2</sub> /y/ (> /i/ in the first syllable), or else it has shifted to the C<sub>2</sub> position in the first syllable, converting /i/ to /u/. But some of these verbs have dialectal by-forms with the vowel /i/ instead of /a/, and the initial W-coloration, which is still admissible in certain dialects, remaining. Here is the complete list: *waiway-* 'turn head', *baibay-* 'wrap thatch round conical roof-frame', *daiday-* 'strip off (skin) with knife',<sup>9</sup> *gaigay-/gwiigwiy-* 'nibble', *lailay-/lwiilwiy-* 'knead (into balls), shake together, smooth over etc.', *mainmayii/maimai*<sup>10</sup> 'going up and down farm doing second weeding, repetition', *rairay-/rwiirwiy-* 'riddle, sift', *saisay-/sausay-/swiiswiy-* 'shear (sheep), crop (mane), cut (hair)', *zaizay-* '(water) lap round, erode, (hair) drop out (round head)', possibly also *taitay-* 'limp along, help tired traveller' and *tsautsayii* 'the vicissitudes of life, an accident'. The group also includes three disyllabic non-geminate verbs with the /w/ in coda position, viz. *juuy-* 'revolve, turn, change', *lauy-* 'bend into arc, turn round and go back' and *sauy-* 'change, exchange, alternate', perhaps also *tauy-* 'shrink, reduce, put tuck in', and one quadrisyllabic with an additional prefixed syllable, viz. *dabaibay-* 'hobble (horse), entangle'. One may compare, too, *musaay-* 'exchange, barter'.

These phono-semantic groups have been illustrated so far entirely from the grammatical class of verbs. This does not mean that they are entirely confined thereto. Such 'deep grammar' categories of the language in fact cut across surface grammar divisions. Witness the following words, which are nouns pure and simple without any cognate verbs:

Group (i): *Fatsaa* 'fishing with hook and line', *kootsoo* 'type of drum played by pressure of the fingers', *tsaatsaa* 'rust', *yaatsaa* 'digit, finger' (which functions by pressure), *hantsakii* 'pincers, tongs'.

Group (ii): *Barzaa* 'lower part of wall liable to erosion', *geezaa* 'mane, fringe'.

Group (iv): *Darbaa* 'a sticky juice or blight in plants'.

<sup>8</sup> These verbs (of whose geminate structure there are other examples, e.g. *sassaƙ-* 'work with adze', *sussuk-* 'thresh', *tsattsag-* 'tap, ram, peck') must be distinguished from plural (or intensive) verbs in which the whole, or part, of a simplex base is repeated, e.g. *gaigay-* 'tell a number of people or a number of times', *saisay-* 'buy a number of things', *kwankwant-* 'undo a number of things', *ragargaz-* 'break into many pieces', *kakkary-/karairay-* 'snap a number of times or a number of things'.

<sup>9</sup> This is the meaning of grade 2. The grade 1 of this verb has the meaning 'pour a few drops of something over something else'. Neither grade has, I think, anything to do with *day(a) day(a)* 'one at a time, in ones and two's', as the dictionaries suggest for grade 1, the derivative verb for *dai dai* being *dai dait-*.

<sup>10</sup> Even where, as here, whole words, not simply bases, are cited, tones are not marked in this section, as being irrelevant to the argument.

Group (vi): *'iskaa* 'air, wind', *'izgaa* 'horse tail; fly-switch'.

Perhaps also *fiskaa/fuskaa* 'face' (with its rapid changes of expression).

Group (vii): *Faifai*, *taimai* 'circular shaped lid mat, disc', *kwai* 'egg', *kwaayaa* 'grain of corn or rice', *gwai-waa* 'testicle(s)', *kaiKayii* 'chaff, dust that swirls around in the wind', *wuyaa* 'neck' and three other rounded, but paired, parts of the body which all have a root *Cwi/uy-*, viz. *gwii-waa* 'knee' (root *gwi/uy-*, pl. *guw-aa-w-uu/gwiy-aa-y-uu*), *duwai(-niyaa)* 'buttock' (root *dwi/uy-*, pl. *duw-aa-w-uu*) and *tsuwee/tsuyee* 'testicle' (root *tswi/uy-*, pl. *tsuw-aa-w-uu*). These last three words constitute a little homo-morphic, homo-semantic group of presumably great antiquity.

Other examples of phono-semantic inter-relation of nouns and verbs can be found in some of the smaller groups, for example the /r/ plus /m/ signifying holes and hollow objects and actions connected therewith, e.g. *raamii* 'hole, pit', *kwarmii* used in *mai kwarmin* 'idoo 'having deep-set eyes', *turmii* 'mortar', *kwarmaa* 'gauge of ring etc.', *kurinii* 'wooded gully'; *burm-* 'invert calabash on another, (earth, roof, mouth) cave in', *burm-* 'stave in', *durmiy-/dulmuy-* 'sink deep, immerse', *firmsits-* 'force way into crowd' (for the -ts- see group (i)), *tsarm-* 'dilute', *zurm-* 'plunge hand into pocket, foot into hole etc.'. Compare too the ideophones *burum* and *burum* applied to distended stomachs, voluminous clothing, toothless mouths etc. In *gurbii* 'indentation, socket, groove' the /m/ is de-nasalized.

Nouns, however, have also their own independent phono-semantic groups. One such large group, 'abstract nouns of sensory quality' (presumably a very old, if not original, category, comprising in all some sixty words) I wrote about at length many years ago (Parsons 1955). These are as much a morpho- as a phono-semantic group, though their phonology also exhibits certain interesting gaps, e.g. the non-occurrence of VOICED (as opposed to voiceless and glottalized) obstruents in C<sub>2</sub> position, which may have historical and genetic implications. Another, much smaller, group is what I term the 'pairs and pieces' class of noun. These are nouns of geminate structure having a base *Ci/uC-*, the C<sub>2</sub> being most frequently a velar consonant, but also /l/ and /m/, which are unique in the nominal vocabulary in manifesting THREE flexional forms which were originally singular, dual and plural respectively, though there is now some confusion of reference and in several cases one or more of these three forms is missing.<sup>11</sup> They all signify either paired parts of the body, or objects occurring in or exhibiting pairs, or bifurcated objects, or objects consisting of two parts. Some of them have cognate verbal roots.<sup>12</sup>

Here is the extant list:

<sup>11</sup> Some Hausas, however, still confine the application of *fiffikee* to a single wing, *fikaafikii* to a pair of wings (e.g. of a bird or fly) and *fukaafukai* to the wings of a number of birds or flies, or the four wings of a butterfly or moth.

<sup>12</sup> When a verb and a noun share the same radical shape and basic meaning, but the noun (i) has a form, singular and/or plural, that is not typical of a verbal noun, and/or (ii) signifies the implement wherewith the action is performed—as opposed, e.g., to the concrete result of the action,—I regard

sing.	dual	plural
<i>did-dig-ee</i> / <i>dud-dug-ee</i>	<i>dig-aa-dig-ii</i>	<i>dug-aa-dug-ai</i> 'heel'
<i>fif-fik-ee</i> / <i>fuf-fuk-ee</i>	<i>fik-aa-fik-ii</i>	<i>fuk-aa-fuk-ai</i> 'wing' (cf. <i>fuffukaa</i> 'flapping of wings')
<i>mum-muk-ee</i> 'one half mandible'	<i>mu-kaa-muk-ii</i> 'the whole mandible'	<i>muk-aa-muk-ai</i> 'whole jaws'
_____	<i>tik-aa-tik-ii</i>	<i>tuk-aa-tuk-ai</i> 'calf, shin'
<i>shis-shik-ee</i>	_____	<i>shik-aa-shik-ai</i> } 'forked post, beam
<i>jij-jig-ee</i>	_____	<i>jig-aa-jig-ai</i> } supporting roof' (cf. <i>shik-</i> = <i>sak-</i> 'put' and <i>jijig-</i> 'shake to and fro')
<i>tsit-tsig-ee</i> / <i>tsut-tsug-ee</i>	_____	<i>tsug-aa-tsug-ai</i> 'stump of tree or tooth (often forked)' (cf. <i>tsig-</i> in ideophones emphasizing shortness or smallness)
<i>zuz-zug-ee</i> 'one skin of a pair of goatskin bellows'	<i>zug-aa-zug-ii</i> 'a pair of bellows'	<i>zug-aa-zug-ai</i> (cf. <i>zug-</i> 'blow up (fire)')
_____	<i>fil-aa-fil-ii</i> 'one oar or single- bladed canoe-paddle, two horns of chief's turban' <sup>13</sup>	<i>ful-aa-ful-ai</i> (cf. <i>filfil-waa</i> 'fluttering')
_____	_____	<i>kil-aa-kil-ai</i> <sup>14</sup> 'the Longtailed Glossy Starling, <i>Lamprotornis caudatus</i> ' (which has a symmetrically gradu- ated tail)
_____	_____	<i>kul-aa-kul-ai</i> <sup>14</sup> 'two loops on portable ink-pot, pair of pompons on sword- sling etc.'
_____	<i>kum-aa-kum-ii</i>	<i>kum-aa-kum-ai</i> 'type of corslet made in two halves'

Morphologically this 'pairs and pieces' class is considerably extended, but the other words in it have various vowels and C<sub>2</sub>'s and they signify either pieces of stuff or

the noun as being a cognate, not a derivative, of the verb. Other examples are *'askaa* 'razor' and the verb *'askèe* 'shave' (sing. form verbo-nominal, but neither plural form *'asàakee*, nor meaning); *turkèe*, pl. *turàakaa*/*turaakuu* 'tethering-peg' and verb *turkèe* 'tether' (neither sing. nor plural form of noun verbo-nominal, nor its meaning).

<sup>13</sup> It is curious that neither the Hausas nor any of their neighbours today, so far as I am aware, use either a double-bladed paddle, or a pair of oars.

<sup>14</sup> For an explanation of this phonemic distinction see Parsons (1970), 284-285.

artifacts not necessarily found in pairs ('rain-clouds, logs, pebbles, crumbs, spindles, lid-mats, hour-glass drums' etc.), or animates ('babies, puppies, pelicans, spiders, concubines'), or else they are purely verbal nouns ('mistakes, stumbles, wood-shavings, pot-scrapings').

I could cite numerous other examples of this strain of phono- and morpho-semanticism running through the language, but will content myself with just one more instance exhibiting a remarkable parallelism in which all three components of the root, C<sub>2</sub>, C<sub>1</sub> and V, play a semantic part, the two consonants having a combined significance, viz.

*tsim-/tsum-* 'put into liquid, soak, steep'

*zir-/zur-* 'put into/through hole' (e.g. bucket into well, thread through needle) or 'put thing with hole in it on to something else' (e.g. needle over thread, ring on finger)

*tsoom-* 'dip into liquid'

*tsaam-* 'extract from liquid'

*zaar-* 'pull out of hole' (e.g. hand from pocket, knife from sheath), or 'pull thing with hole in it off other thing' (e.g. ring off finger).

And this initial /ts/ of liquids appears again, combined with the C<sub>2</sub> /ts/ of pressure, in *tsoots-* 'suck' and *tsuutsaa* 'worm, grub, maggot' (which is probably in origin a v.n. of the former), and with the /rm/ of holes in *tsarm-* 'dilute, water down'; whilst the /z/ and /r/ of holes is also seen in the abstract noun of sensory quality *zurfii* 'depth (especially of wells)', in *zungur-* 'poke, prod (esp. stick into hole)' and *zunguruu* 'the long, hollow gourd worn by women to protect hennaed hands' (also in *zurm-*, cited above).

The question now is, what inferences are we to draw from this marked strain of phono-semanticism in Hausa vocabulary?<sup>15</sup> Are we for instance to assume that one or two words only in each group represent original Hausa (Chadic?) roots, and that the other words were later coined from these by those processes of analogy that are so strong in language (e.g. 'slum' and other 'sl-' words in English)? Or can we take it that the groups as such are of equal antiquity, and all the words in them were formed on the basis of some primitive principle of 'deep' semantics? To try and answer this question, we should look for analogies in other languages. A certain amount of phono-semanticism is clearly to be found in ALL languages, amounting in extreme cases to genuine onomatopoeia, and with the comparative paucity and, to a large extent, uniformity of speech sounds in all languages, it is not surprising that some

<sup>15</sup> So far as I am aware, the only previous reference to this phono-semantic strain is in Gouffé (1966).



sounds and combinations of sounds should have what may well be universal associations. An obvious example is the final velar consonant of so many verbs indicating acts of impact, impingement, or incision, e.g. Hausa *yank-* 'cut', *sook-*, *cak-* 'stab', *dook-*, *raak-* 'beat', *hak-* 'dig', *dak-* 'pound', *nik-* 'grind', *fark-*, *bark-*, *yaag-* 'tear, rend', *tsaag-* 'incise, split', *tsik-* 'crunch', *bug-* 'hit, beat', *tsiikar-* 'tickle'; Common Bantu *-pùk-*, *-bùg-* 'dig', *-ciag-* 'grind', *-kèk-* 'cut', *-bádik-*, *-yápuk-*, *-yátik-* 'split', *(-bààg-)* 'tear', *-pùàg-*, *-túàng-* 'pound';<sup>16</sup> Swahili *piga* 'hit'; English 'hack, stick, prick, poke, flick, strike, dig, hug, clog, bang, whang, sting'; Latin *sec-*, *plic-*, *ping-*, *pung-*, *string-*, *frang-*, *tang-*, *cing-*; or the */um(b/p)/* of large, roundish things or actions, e.g. Eng. 'hump, dump, rump, lump, plump, clump, cumber, lumber, tumble, rumble' ('chump, frump, sump' are pejorative); Hausa *kumbur-* 'swell, puff up', *rungum-* 'take armful of, embrace', *lumbuu*, *luntsum-* 'plump', *hunkum-*, *kunkum-* 'huge', *dungum-* '(number of persons) move off in a body', *sungum-* 'lift large or heavy object', *gungumee* 'log', *tumbii* 'stomach (of ruminant)', *rumbuu* 'round corn-store'; Common Bantu *bùmb-*, *-tùmb-* 'swell', *-bùmb-* 'mould pottery', *-bùduŋg-* 'become round', *-bùmò*, *-tùmbù* 'abdomen';<sup>16</sup> Latin *tumeeo*, *tumulus*, *cumulus*, *lumbus*, *rumis*. Even more universal perhaps is the onomatopoeic shape *(s)p/fi/u(t)*, or *ti/u(f/p)* meaning 'spit'—Common Bantu *-tù-*, *-tùfj-*.<sup>16</sup> Hausa has a stronger vowel here in the verb *toof-*, but its expletive ideophone is *tuf*. Indeed it will already have been observed that a number of the Hausa verbs given in the phono-semantic groups above are remarkably like English verbs of similar meaning, e.g. *burts-* and *burst*, *tirts-* and *thrust*, *burg-* and *brag*, *gurz-* and *graze*, *wuzg-* and *whisk*, *tuug-* and *tug*, and there is even a perfect example of a 'backwards and forwards' verb with */w/* and */y/* in English *sway*,<sup>17</sup> whilst Hausa *sharb-* 'drink noisily' is immediately reminiscent of Arabic *shariba* 'drink', and Hausa *toots-* of German *stossen* 'push, thrust', or Hausa *gurbii* of German *Graben*. There are in fact a great many more Hausa-English resemblances outside the words cited, some of them phonetically so close as to preclude any possibility of their being genetic cognates, e.g. *zuuk-* 'suck in/up', *sulb-*/*sullu* *b-* 'slip off' and ideophone *suluf*, *dirk-* both 'strike' and 'drink', *bincin-* 'take a pinch of', *wandar-* 'deviate, wander', *firgit-* 'frighten', *leeɓɓe* 'lip', *fuskaa* 'face', *hannuu* 'hand', *gurungurtsi* 'gristle', *bitaabitai* 'little bits', and a whole little group of nearly synonymous nouns like *huuluu* 'fool', *dooloo* 'dolt', *lootoo(too)* 'lout', *gaulaa* 'gowk', and (for American English) *gaaboo* 'gaby' and *saakarai* 'sucker'.

Much of Hausa vocabulary in fact is strikingly Indo-European, or, more precisely, Germanic in its appearance.<sup>18</sup> And it would seem that the two sets of words present similar exegetical problems. On the one hand there are a great many words in Germanic languages, particularly among the more expressive and phono-semantic

<sup>16</sup> These are all taken from the index of radicals at the end of Guthrie (1967 sqq.), part II.

<sup>17</sup> Compare too Latin *volvo*.

<sup>18</sup> I once compiled a list of more than 100 Hausa words with phono-semantic counterparts in English and German. For further examples, see Tucker (1957), 550.



verbs, which have no counterparts in any other branch of Indo-European.<sup>19</sup> And on the other hand very few of the many Hausa words that I have cited above have any cognates in other Chadic languages, at least very few are cited by Newman and Ma (1966).

Verbs as a whole comprise less than a third (45) of the 145 'proto-Chadic lexical items' given at the end of Newman and Ma, and not all of them have citations from Hausa. Most of those that have are basic verbs of quite general meaning such as 'eat, drink, give, go, die, say, know', which are not the sort of verbs one would expect to exhibit any phono-semanticism in any language. The only exceptions are: no. 30 *\*t-d-* 'to fall', for which is cited, not Hausa *faad-* 'fall', but *taad-* 'trip' and with but one cognate (presumably meaning 'fall') *tede* in Margi; no. 79. 'to pound grain' *\*s-(r)p-*, for which is cited Hausa *surf-*, not the ordinary word for 'pound' (see the 'liquidity' group above) and again one cognate only *sūpa* in Tera; no. 134 'to hoe' *\*h-d-*, for which is cited Hausa *hūd-* (which again does not mean strictly 'hoe', but 'bank (soil) round plants'), and again but one cognate, Tera *ghude*; and no. 122 'to beat' *\*d-G-*, which is the only one for which roots from a number of languages are cited, including Hausa *dook-*, but which, as I have suggested above, is too obvious in its /k/g/ implication to claim as a phono-semantic word limited to a particular group of languages. So we are really left guessing as to how the Hausa language acquired all these phono-semantic verbs. But, if they are original, as I myself am inclined to believe, then the presumption on the Chadic hypothesis must be that they occurred also in proto-Chadic. In which case they must surely have left more traces in the form of cognates in other Chadic languages than is apparent from this list. I suggest that it should be one of the prime tasks of Chadic comparativists to look for such cognates. Or at least they should look for examples in other Chadic languages of the same strain of phono-semanticism as is so apparent in Hausa, by making internal groupings as I have done for Hausa. It may well of course take other forms, and with other sounds, in other Chadic languages, but at least it might be established as a general principle of the family, or, conversely, as being confined to some languages only, or unique to Hausa. It should be added that one of the nouns also, from my 'pairs and pieces' group, figures in the comparative list, viz. *fikāfiki* (cited in that form) under no. 19 'wing' *\*(k)p-k*, with again only one cognate, the Tera *kopax*. This is reasonable enough in isolation, but one would like here to know (a) whether the Tera word has the same trinary scatter of forms as the Hausa word

<sup>19</sup> Two opposite theories are held by Germanic scholars concerning these words, and this is very relevant to the Hausa-Chadic question. I quote from Priebsch and Collinson (1958), 281: "There is in Primitive Germanic a residue of untraced words which have led some scholars like S. Feist to postulate a non-I-E strain in the early vocabulary, but in some cases it would be safer to assume the loss of a word in other I-E languages rather than its non-existence within the family ... . However it must be admitted that even some of the commonest Germanic words have so far baffled the etymologist, e.g. *breit*, *bringen*, *dumm*, *halten*, *Luft*, *Regen*, *treiben*, *trinken*, *See*, *Klippe*, *leck* and *Kahn*".

(discounting the matter of reduplication); (b) whether the Tera words for 'heel', 'jaw', 'shin' and/or other paired parts of the body form any sort of morpho-phonetic group with *kopax*.

Going beyond the Chadic group to look for possible cognates in other Hamitic or Afro-Asiatic languages, we find Meinhof (1912) even less rewarding. Of the 78 items given in the comparative wordlist at the end of his *Die Sprachen der Hamiten*, less than a third again (23) are verbs, and these in the main cover the same general terms like 'eat', 'drink', 'see', 'speak', 'know' as does Newman and Ma's Chadic list. Discounting the universal onomatopoeia 'spit', in fact, we find only one of the Hausa verbs I have cited, viz. *murd-* from group (iii), and none of the few suggested cognates in Berber, Somali etc. has more than the first two (less significant) consonants of the Hausa root.

Much more rewarding, however, is a recent article by Hodge (1966). He gives a list of 101 items (some of which are multiple, i.e. include variant shapes or variant meanings in one or the other language), of which exactly a half represent verbs in one language, or both, the majority of them having concrete and rather specialized meanings. They include a number of very common Hausa verbs which do not constitute members of any very obvious phono-semantic group, e.g. *fark-*, *harb-*, *jir-*, *koor-*, *keer-*, *koos-*, *kum-*, *rak-*, *saar-*, *tsaar-*, *tsan-*. But they also include nine of my phono-semantic verbs, no less than seven of them (*daats-*, *gaats-*, *laats-*, *mats-*, *raats-*, *roots-*, *waats-*) from the 'pressure' group. Moreover the 'established' Egyptian cognates of all these words display a similar phonic homogeneity with *ǧ* as their second (in one case the third) radical. Unfortunately, however, the Egyptian words, as here glossed, are much less homogeneous semantically, and they have no obvious idea of pressure running through them. Thus corresponding to *gaats-* 'bite' we are given Eg. *rǧ* 'hack up, destroy', to *mats-* 'squeeze' Eg. *m ǧ ḥ* 'fillet', to *raats-* 'cleave through' (here cited in its semantically deviant grade 4 meaning 'swerve off (road), bypass (town)') the Eg. noun *ǧ t* 'cobra', and to *roots-* 'break (head)' the Eg. adjective *r w ǧ* 'hard'. Furthermore the Eg. *n ǧ* 'grind', which is cited as corresponding to Hausa *laats-* (which should have been glossed as 'squeeze GENTLY'), is both semantically and phonetically much closer to Hausa *nik-* 'grind'. There are also a couple of others of my phono-semantic verbs cited in Hodge's list of established cognates, viz. *harg-* in the nominal derivatives *hargoowaa* and *hargaagii* 'uproar', which are equated with Eg. *ḥ ǧ g* 'be pleasant, glad' and *ḥ ǧ g ǧ g* 'exult', and *sharb-* 'drink noisily', which is equated with Eg. *š ǧ b w* 'meals'. But these are isolated examples of their phonological patterns in both languages in the list. The same is true of *tsart-* 'spit out in a stream', equated with Eg. *ǧ ǧ ʾ* 'extend (arm), pierce', in which the initial *ts-* may have some link with the little *tsim-/tsoom-/tsaam-* group I have cited above, and *sark-* 'interlace' equated with Eg. *s ǧ q* 'pull together', where the */rk/* MAY also represent a phono-semantic group, albeit a small one: compare *harkum-* '(wrestlers) clinch', *burkum-* (wrestler) tangle with (and throw) opponent' and *tarkookoo* 'difficulties, entanglements'.

In a subsequent article (Hodge 1968) he cites a number of other common Hausa verbs which do not appear among the Hausa-Egyptian established cognates, e.g. *dir-*, *fas-*, *kif-*, *kad-*, *tsuul-*, but these do not figure in any obvious phono-semantic groups (except perhaps for the *ts-* of *tsuul-* 'pour', again signifying liquids), and the meanings of their supposed cognates in other Afro-Asiatic languages is so varied as to be of doubtful comparative value. One other Hausa verb cited, viz. *dann-* 'press down on, hold down, close tightly, force', however, is more significant, since this is a member of one of the smaller groups, having the rare (in verbs) geminate combination /*nn*/, which would seem to signify PROLONGED pressure, cf. *mann-* 'stick t. on adhesively', *kann-* 'screw up one's eye to look fixedly at t., grit one's teeth, steel oneself etc.', and *kunn-* 'apply (fire etc.) to t.'. And there does seem to be a common element of meaning in both the Eg. *dn* 'hold back, restrain, dam (water), revet (banks)' and the Berber *dounet* 'se tenir rassemblé sur soi-même'; but again it is an isolate. I should add that one other Hausa verb cited in this list, viz. *'uufèe* (only used in this grade) 'take to one's heels', which Hodge compares with Ar. *'afida* 'hurry' and Eg. *'fd* 'flee', is clearly an onomatopoeia: in fact the base *'uwf-* is a metathesized shape of the common ideophone *wuf* signifying sudden rapid movement of any sort (e.g. *Ya yi wuf ya fìta koofàa* 'He rushed out of the door'), and both the ideophonic and the verbal shapes are phonologically anomalous, for (i) an inherently W-coloured (or minus coronal) C<sub>2</sub> in a *y/wVC* base induces a contrastive Y-coloured (or plus coronal) manifestation of C<sub>1</sub>, i.e. *yif* (cf. verb *yifàa* 'throw on carelessly'), (ii) the /*i/u*/—unlike the /*a*/—vowel phoneme does not in native words occur in a CVCC- base whose C<sub>1</sub> is /*'*/.<sup>20</sup>

There would seem therefore to be almost no systematic comparative evidence for relating the vocabulary of Hausa to that of any other language, ancient or modern, at least nothing approaching either the 2,000 starred forms of Common Bantu, or the 1,000 odd starred forms of Indo-European, and less of it with verbs than there is with nouns (contrast again Bantu and Indo-European), and noun comparisons are far more suspect, since cross-cultural loaning is so much commoner with nouns than it is with verbs. What evidence there is, however, does seem to point to a closer connection between Hausa and Egyptian (possibly also between Hausa and Berber) than it does between Hausa and any of the Chadic languages at present recorded. This I may add is in line with my own 'hunch' about Hausa, viz. that it is the sole surviving descendant (with the possible exception of Gwandara—a small and neglected language which is crying out for intensive study) of a lost Saharan language (or group of languages) which was fairly closely related to Ancient Egyptian, and that its present-day geographical propinquity to the Chadic languages (which has undoubtedly been a factor in its grouping therewith) is purely fortuitous.

<sup>20</sup> See Parsons (1970), 275 n. 8, 285 n. 39, 278-279.

## III. MORPHOLOGY

I turn now to morphology. Here Hausa today presents us with a remarkable paradox, viz. on the one hand a highly complex, heterogeneous and dialectally variant system of inflexion for the noun; on the other an almost mathematically simple, homogeneous and dialectally non-variant system of inflexion for the verb (including with it the verbal noun). By the former of course I refer to the noun pluralization system, by the latter to the verb grade system (see Parsons 1960). The extreme morphological simplicity of the latter—consisting as it does (apart from the somewhat peculiar grade 5) of mere combinations and permutations of three basic tone-patterns and seven terminal vowels (*-a*, *-aa*, *-e*, *-ee*, *-i*, *-oo*, *-u*)—has struck students of the language from the earliest times, especially in contrast to the complex verbal morphology of Chadic and some other African languages. Notionally, it must be admitted, there is probably nothing unique about the system, unless perhaps it be in the multi-functional Grade 2 of the Hausa verb. ‘Adessive’ and ‘completive’ forms of the verb, for instance, are to be found in Chadic and other languages, and there are even purely syntactic distinctions of form in at least one Chadic language, Bolanci.<sup>21</sup> But in these other languages such distinctions of meaning are conveyed by systems of affixes involving additional consonants, and/or by internal vowel mutation. Moreover these are mixed up with exponents of person, tense and aspect, whereas in Hausa the latter are all preverbal and quite distinct from the various grade forms, all of which may occur in any tense (including the imperative).

The two most striking features of the system are perhaps (i) its all-pervasiveness; (ii) its internal autonomy. By the first I do not only refer to its dialectal uniformity, but even more to the way that it embraces the whole verbal lexicon. This is in marked contrast to verbs in most other inflected languages, European and African, with their numerous different ‘conjugations’ (in African languages often represented by different tonal classes) and numbers of ‘irregular’ verbs. In Hausa there are no more than one per cent of irregular verbs, and these are irregular only in one or two of their grades (or in neutralizing primary grade distinctions). Nor is any distinction made between indigenous and loan vocabulary. Every single loan verb, Arabic or English (there are—for a special reason—not very many of the latter), has been made to conform to the Hausa grade system with a positively Procrustean relish; and it is not without significance that the only verb I know that operates all seven grades in full is the Arabic (via Kanuri) *karant-* (see Greenberg 1960) ‘read, study, teach’, and that the English loan verb *canj-*, which has only come into the language in the last half century (but is threatening to supplant two or three native verbs), meaning ‘change, exchange, transfer, take over (from), relieve etc.’ operates six of them.

<sup>21</sup> See J. Lukas (1964), 169. However, Carnochan (1970), 87-90 (“Transitivity”) gives some examples of purely tonal syntactic distinctions, such as are to be found in Hausa.

As to the internal autonomy of the system, though notionally speaking it is hierarchical, certain grades being primary, others secondary and others tertiary, there is no morphological evidence of any one form having been derived from any other, or indeed of any of the forms being anything but original—at least as a potential for all verbs, though some grade forms of some verbs were probably evolved later than others, e.g. *ciyoo/ciwoo* 'eat and come, etc.' (see Parsons 1970, 287). The whole system in fact is far too tidy to have been derived from any other language: at least it is my belief that it is extremely primitive, conservative and—among living languages—unique to Hausa.

If we extend the morphological system to embrace verbal nouns as well as finite verbal forms, the language presents an even more mathematically complete picture, with verbal noun forms occupying most of the spaces in the tone-pattern-termination chart left unoccupied by verbal forms (with a few ambivalent forms as well). Thus verbal nouns have all the following terminal vowels: *-aa*, *-e*, *-ee*, *-i*, *-ii*, *-oo*, *-uu*, plus the two diphthongs *-au* and *-ai* (the last is rare), and the majority of these occur with two or three different tone-patterns to give a total of eighteen disyllabic and at least thirty polysyllabic forms. But, unlike finite forms, (i) few of these verbal noun forms have a unified, specific function or meaning, (ii) there is some synonymous dialectal variation of form, (iii) the totality of forms is not a potential of the whole verbal lexicon, some forms being restricted to certain verbal bases only and determined by phonological, rather than functional or semantic, factors. And there is a further complication in the base-ablauted forms, such as *jiifàa* from *jeef-*, *duukàa* from *dook-* and *kisàa* from *kas-*, which are a feature almost peculiar to the verbo-nominal system in Hausa that is at once both phonologically determined and to some extent phonologically aberrant.<sup>22</sup>

When we turn to the nominal system, the picture at first glance is an entirely different one, viz. a welter of completely unsystematized basic, or singular, forms in which both tone-pattern and termination are lexical, not grammatical, differentiae,<sup>23</sup> plus a considerable number of plural forms, some of a high degree of morpho-

<sup>22</sup> Parsons (1970), 277, n. 17. In Chadic languages such basic vowel ablaut seems to be a feature of the finite verb rather than of the verbal noun. In Bachama for example it is an exponent of number, see Carnochan (1970), 101-103.

<sup>23</sup> But see p. 452 of the present article. There is also in Hausa a peculiar phenomenon, which is too widespread in its incidence to be merely fortuitous, whereby, when a verb and a noun share a common homophonic (but not cognate—see note 12) base, the noun almost always has a combination of termination and tone-pattern that is not found in the verbal (though it may be in the verbo-nominal) system, e.g. verbs *daamàa* 'mix, confuse', *dàamaa* 'annoy', noun *daamaa* 'leisure, opportunity'; verbs *rinàa* and *rinaa* 'dye', noun *rinaa* 'hornet'; verb *dookàa* 'beat', noun *dooklii* 'horse'; verb *keerèe* 'overtop', noun *keeree* 'throwing-stick'; verb *keesoo* 'cleave way through', noun *keesòo* 'old mat'. In a few cases a noun also fills a lexical gap in the verbal system, e.g. verb *kaasàa* 'fall short, fail', nouns *kàasaa* 'sort of blanket' and *kaasaa* 'puff-adder'. There are some complete verb and noun homophones, however, e.g. *dookàa*, *dòokaa*, *shèekaa*, *kaawoo* and *koomoo*. Noun plural forms too—at least polysyllabic ones—are always morphologically unambiguous, e.g. *randunàa*, pl. of *ràndaa*

logical complexity, involving infixation, suffixation and reduplication, as well as the mere mutation of terminal vowel and/or tone-pattern (as with the verbal system). Two or more of these processes often operate at once, e.g. *duw-à-r-w-a-ts-uu*, pl. of *duwts-èè* (flexion, infixation and reduplication), *ciw-i-r-w-i-t-aa*, pl. of *cùut-aa* (< \**ci(i)w-t-aa*) (tonal flexion, infixation and reduplication), *gar-èè-w-anii*, pl. of *garw-aa* (infixation and suffixation), *koof-oo-f-ii*, pl. of *koof-àà* (flexion, infixation and reduplication), *màgàn-gàn-uu*, pl. of *màgan-àà*, *shaawàr-war-ii*, pl. of *shaawar-àà* (flexion and reduplication), *'ab-uw-b-uwàà*, pl. of *'àb-ii/aa/ù*, *buh-un-h-unàà*, pl. of *bùh-uu*, *kaay-ây-y-akii*, pl. of *kaay-aa* (tonal flexion, suffixation and reduplication, including reduplication of part of the suffix). In a few cases there is internal ablaut as well as flexion, e.g. *maruk-àà*, pl. of *màrak-ii/aa*, and this may be combined with reduplication as in *maagun-gun-àà*, pl. of *maagàn-ii* and *kabur-bur-àà*, pl. of *kabàr-ii*.

These various plural forms can, however, for the most part be systematically related to their singular forms, though the 'rules' are exceedingly complicated, involving all the following hierarchically ordered criteria (in the singular form): (i) number of syllables, (ii) tone-pattern, (iii) terminal vowel quality, (iv) root-termination vowel sequence (e.g. *CaaC-aa* and *CaaC-oo*), (v) weight of sole or final syllable of the root (CVC or CVCC), (vi) presence or absence of 'W prosody', (vii) nature of coda consonant, esp. /n/ or /r/. And there are many exceptions to the rules in the case of individual words, both minor ones (change of sub-class, or adventitious reduplication) and total or heteroclite ones (singular in one class, plural in another), not to mention some unique 'freak' plurals such as *'àbòokànai*, *'àbàibàdai*, *tsummoo-karàà*, *raggadiudàà*. Moreover, (i) although the plural forms of most commonly used nouns are now more or less standardized, there are numbers of dialectal and idiolectal variants occurring in written as well as in spoken Hausa; (ii) a number of common nouns have—either 'by the rules', or by convention—a choice of two or more plural forms even in standard Hausa, e.g. *miyàaguu* and *muḡḡaa*—besides a heteroclite *muugàayee*—pl. of *muugùu*, *làabàarai* (reg.) and *làabàaruu* (het.), pl. of *làabaarii*, *fitiluu* and *fitiloolii*, pl. of *fitilàà*, and these alternative forms are frequently used *ad lib.* by the SAME speaker or writer, though in some cases the two forms may have semantic distinctions, e.g. *turàamee* 'rolls of cloth', but *turmàayee* (partially irregular) 'mortars' (sing. *turmii* has both meanings—cf. 'brothers' and 'brethren'). A combination of these two factors (plus a much rarer dialectal variability in the singular form) has led to a remarkable proliferation of plural forms in the case of certain lexemes, the *ne plus ultra* being *kadàà/òò* 'crocodile', which has no less than ten distinct plural forms recorded in the dictionaries.

'storage-pot', but *rùndunaa*, s. 'army, host'; *kumàatu*, pl. of *kuncii* 'cheek', *kùnàamuu*, pl. of *kùnaa-màà* 'scorpion', but *kuudàkuu*, s. 'sweet-potato', *kudaabiùu*, s. 'hidden depth in river-bed'. (Many plural forms, however, have come to function as singulars, e.g. *'agàanaa* 'smallpox', *kuràadaa* 'chopper', *'iyàakaa* (orig. pl. of *'iikòò*) 'boundaries, limit', *shàddaa* 'cesspit', *gidaa* (pl. of *gijii*) 'compound, home', *kudaa* (pl. of *kujèè*) 'fly, flies', *'itàacee* het. pl. of *'icèè* 'wood, tree', *màtsàtsàkuu* 'leech(es)', *dàngàràfai* 'clog(s)', *kàlànguu* 'hour-glass drum', *kwàalàatai* 'testicles').



Now what are we to say about all this morphological profusion in the historical and comparative perspective with which this paper is concerned? First of all it should be made clear that this multifarious—call it ‘primitive’ if you like—system is very far from obsolescence, though there are signs in modern Hausa speech and writing that a certain reduction and levelling is beginning to take place, as it did in English some centuries ago. But that the system is almost as innate to the language as is the verbal grade system is shown by the fact that the great majority of English nouns taken in during the last few decades have standard plural forms evolved—by analogy—more or less strictly in accordance with the ‘deep rules’. They occur in almost all the classes (except—for obvious reasons—the level-toned singular ones),<sup>24</sup> including some of the smaller ones, e.g. *kuràataa*, pl. of *kurtùu* ‘recruit’, *siyàasaa* or *sišsaa*, pl. of *siisi* ‘sixpence’ (Eng. via Yoruba), *suluuluwàa/sulullukàa*, pl. of *sulèe* ‘shilling’, *laayuyyukàa*, pl. of *laayii* ‘line, row of market-stalls, street etc.’, *footunàa* or *fòotàanii*, pl. of *fòotoo* ‘photo, picture’ (not to mention the classic *sùkùruudiree-boobii* ‘screwdrivers’!). One such indeed has recently been coined in Kano by a certain vendor of a new style of (native) footwear on the good old ‘pairs and pieces class’ (see section 2 above) model, viz. *shùwàashùwai*, sing. *shuù* ‘(pair of) shoes’. Several loan nouns, too, are multi-class like many native nouns, e.g. *kwabòo* ‘penny’ (Eng. ‘copper’), pl. *kwàbbai*, *kwabbunàa*, *kwabumbunàa*, *kwabàbbaa* (rare), *kwàbàa-kwàbai* (cf. native *tabòo* ‘scar’, pl. *tàbbai*, *tabbunàa*, *tabumbunàa*, *tabàbbaa* and *tabbaa*). But how old is the system and whence did it originate?

As I said in my previous article, my knowledge of Chadic languages is superficial, and I have made no special comparative study of the noun pluralization processes or morphemes in any of them. But my impression is that these are on the whole simpler and fewer in number and variety in any one language than they are in Hausa; from which of course it may be argued that these languages are less conservative than Hausa (compare, e.g., English versus German). On the other hand there are clear parallels between Hausa plurals and plurals in some other Afro-Asiatic languages, notably regarding reduplication and the *-aa-* that is the commonest infix vowel (e.g. *kas-aa-shee*, *’as-aa-k-ee*, *sul-aa-l-aa*, *kur-aa-t-aa*, *kaf-aa-f-uu*, *miy-aa-g-uu*, *fik-aa-fik-ii*) (see Greenberg 1955), and of course in the root-disjointed (as I prefer to call them) plurals such as *’as-aa-k-ee*, *lak(w)-aa-n-ee*, *tur-aa-m-ee*, *kur-aa-t-aa*, *siy-aa-s-aa*, *miy-aa-g-uu*, *duw-aa-ts-uu*, which are so reminiscent of the broken plurals of Arabic, as is also the plural suffix *-unàa* (though this last is but one shape of a complex morpheme *-unàa/-ukàa/-uwàa* whose three allomorphs are very largely in complementary distribution depending upon the consonantal components of the base form). There is also some resemblance to the ‘substitutional’ plurals of Berber (see Greenberg 1955) in the ablauted plurals of the *marukàa* type, though these in

<sup>24</sup> The only examples of level-toned words among English loanwords are where an English word has been completely assimilated to a pre-existent native word, e.g. *kwalbaa*, pl. *kwalàabee* ‘(1) bottle, (2) crowbar’ (*Taroo* ‘threepence’ is NOT a direct loan from English).

Hausa are confined to polysyllables and affect only the final syllable of the base (unless the preceding syllable is a reduplication).

However, when one comes to study all these plural forms in Hausa more closely, one finds that it is possible to layer them on what may well be an historical, as well as a morphological and lexical basis, roughly as follows:

(1) Those in which pluralization is effected by a mere change of terminal vowel and/or tone-pattern—exactly like the flexional forms of the verb. They may be subdivided further into:

(a) The 'polar' type, i.e. those in which a singular ending *-aa/-oo*, i.e. an open or a half-open vowel, has a corresponding close vowel plural ending *-ii*, and a singular ending *-ii/-ee*, i.e. a close or half-close vowel, has a corresponding open vowel plural ending *-aa* (with or without tonal change). These are not numerous, but they include some of the most basic, and presumably the oldest, nouns in the lexicon, e.g. *baawàa*, pl. *baayii* 'slave', *kàazaa*, pl. *kàajii* 'chicken', *bàreewaa*, pl. *bàrèeyii* 'gazelle', *tàabar-maa*, pl. *tàabàrmii* 'mat', *kùyangaa*, pl. *kùyàngii* 'slave-girl', *zàaboo*, pl. *zàabii* 'guinea-fowl', *kwàadoo*, pl. *kwàadii* 'frog', *tàuraaròo*, pl. *tàuràarii* 'star', *ɓàraawòo*, pl. *ɓàràayii* 'thief'; *mijii*, pl. *mazaa* 'man, male, husband', *màcè/màataa*, pl. *maataa* 'woman, female, wife', *'arnèe*, pl. *'arnaa* 'pagan' (originally probably applied to any non-Hausa African), *hakoorii*, pl. *hakòoraa* 'tooth'.

(b) The 'polar plus' type in which there is, in addition to simple vowel mutation, an extra feature of Y- and W-coloration, which combines with the opposition open/close as follows: open plus neutral or Y-coloration → close plus W-coloration; close plus Y-coloration, open plus W-coloration and close plus W-coloration all → open plus Y-coloration. So singular *-aa/ee*, plural *-uu* (*i/uw*); singular *-ii/oo/uu*, plural *-ai* (*ay*), e.g. *yaatsàa*, pl. *yaatsuu* 'digit', *zàkaràa*, pl. *zàkàruu* 'cock', *màraayàa*, pl. *màràayuu* 'orphan', *tàntabàraa*, pl. *tàntàbàruu* 'domestic pigeon', *maayèe*, pl. *maayuu* 'wizard', *jeemaagèe*, pl. *jèmàagu* 'fruit bat', *kàdangarèe*, pl. *kàdàngàruu* 'lizard'; but *birii*, pl. *birai* 'monkey', *jàakii*, pl. *jàakai* 'donkey', *'àbookii*, pl. *'àbòokai* 'associate, friend', *kòofatòo*, pl. *kòofàtai* 'hoof', *'indararoo*, pl. *'indàràrai* 'drain-spout' (a loanword, see note 20), *tùdùu*, pl. *tùddai* 'upland, hill', *màaduguu*, pl. *màadùgai* 'caravan leader', *tàngàraafù* (Eng.), pl. *tàngàràafai* 'telegraph (pole)'.

(2) Those in which the essential exponent of pluralization is a (usually low-toned) *-aa-* infix (which in one sub-class has disappeared, leaving only tonal traces) inserted either within the root itself ('disjointed plurals'), or preceding a repetition of the final root consonant, the terminal vowel in this case depending not so much upon the terminal vowel of the singular, but upon the tone-pattern and sometimes other phonological features of the latter, e.g. *'askaa*, pl. *'asàakee* 'razor', *turmii*, pl. *turàamee* 'mortar', *kasaa*, pl. *kasàashée* 'earth, land', *wurii*, pl. *wuràaree* 'place, occasion', *karfèe*, pl. *karàafaa* 'metal', *taikii*, pl. *tayàakaa* 'hide sack', *dawòo*, pl. *dawàyyaa* 'ball of flour etc.', *yaaròo*, pl. *yaàraa* (< *\*yaaràaraa*) 'child', *saashèe*, pl. *saàsaa*



(< \*saasàasaa) 'part, region', *zoobèe*, pl. *z(w)abbaa* 'ring', *geemùu*, pl. *gyam̐maa* (< \*geemàamaa) 'beard', *dookii* (< \*daukii), pl. *dawaakii* 'horse', *tumki(yaa)*, pl. *tumaakii* 'sheep', *tufàa*, pl. *tufaafii* 'garment', *duutsèe*, pl. *duwàatsuu* 'stone, rock, mountain', *murfiu*, pl. *muràafuu/muraafuu* '3-stone cooking place', *kafàa*, pl. *kafàafuu* 'leg, foot'.

It should be noted here that (i) plurals of the type *kuuraa*, pl. *kuuràayee* 'hyena', *gwankii*, pl. *gwankàayee* 'roan antelope', *zoomoo*, pl. *zoomàayee* 'hare', *tsuntsuu*, pl. *tsuntsàayee* 'bird', *kwai* (< *kwaayii*), pl. *kwaayàayee* 'egg' may be included in this main group (not, i.e., in the next group) on tonal and other evidence, the -y- (phonetically a mere glide) representing the syllabic initial marker of zero reduplication;<sup>25</sup> (ii) plurals of the now common, and rapidly spreading, pattern -CooCii are clearly of later origin and are simply a phonetic variation of, or innovation in, the -CaaCii type of *dawaakii* 'horses', *tumaakii* 'sheep', and *'awaakii* 'goats' (possibly induced by vowel assimilation in such words as *koofàa*, pl. *koofoofii* 'door/gateway'). The transition is clearly seen with *tufàa* 'garment', which has a later and much less common plural *tufoofii* alongside the standard and older form *tufaafii*.<sup>26</sup> There is another survival of the older shape in the word *karmaamii* 'corn leaves', the singular of which is no longer extant.

(3) Those that have some form of suffix in the plural form introducing an additional consonant not appearing in the singular. These suffixes are (i) -unàa/ukàa/uwàa, (ii) -àkii/-à(i)kuu/-akii, (iii) -ànnii/ànnuu/anii. Examples are: (i) *riigaa*, pl. *riigunàa* 'gown', *tùuluu*, pl. *tuulunàa* 'water-pot', *jàkaa*, pl. *jakunkunàa* 'bag', *kauyèe*, pl. *kauyukàa* 'village', *daajii*, pl. *daazuzzukàa* '(tract of) bush', *zanèe*, pl. *zannuwàa* 'woman's body cloth', *'àbii/'àbaa/'àbù*, pl. *'abubuwwàa* 'thing', *gàrii*, pl. *garuuruwàa/garurrukàa* 'town', *tàafii*, pl. *taafunàa/taafukàa* 'palm of hand, sole of foot', *raamii/ràamii*, pl. *raamunàa/raamummukàa* 'hole, pit', *kai* (< *kaayii*), pl. *kaawunàa/kaayuwàa/kaayukàa* 'head, self'; (ii) *gwaanaa/goonaa*, pl. *gwàa/gòonàkii* 'field, farm', *kwaanaa*, pl. *kwàanàkii* '(spending) night, 24-hour period', *raanaa*, pl. *ràanàkuu/ràanàikuu* 'sun, day (time), day of the week', *gaawaa*, pl. *gaawàwwakii* 'corpse, carcass', *kurciyaa* (< \*kurtaa?), pl. *kurtàttakii* 'dove', *ceediyaa*, pl. *cèedàkuu* 'the tree *Ficus thonningii*'; (iii) *kàakaa*, pl. *kàakànnii* 'grand-parent', *watàa*, pl. *wàtànii* 'moon, month', *kàmaa*, pl. *kàmànnii/kàmànnuu* 'appearance, likeness', *gàrmaa*, pl. *garèemarii* 'trenching hoe', *fàrkaa*, pl. *farèekanii* 'paramour'.<sup>27</sup>

<sup>25</sup> In these level-toned nouns, alone, there is a complete dichotomy between CVC- bases on the one hand, which have full C<sub>2</sub> reduplication, and CVVC- or CVnC- bases on the other hand, which have zero reduplication. It is a curious fact too that /n/ alone of the fully consonantal codas never induces root disjunction in the plural: contrast /m/, as in *kyamree/kyaree*, pl. *kyamàaree* 'door', *damsèe/dantsèe*, pl. *damàatsaa* 'upper arm', *kuncii/kuncii*, pl. *kumàatuu* 'cheek'.

<sup>26</sup> The low-toned termination here is anomalous, all other plurals of types (1) and (2) having high-toned terminal syllables.

<sup>27</sup> The last two examples exhibit an unusual -ee- infix as well, which distinguishes this form from

(4) Those in which, in addition to a fixed termination *-aa*, there is either (a) a change of the second vowel of the disyllabic root from *-a-* to *-u-*, or (b) a repetition (in geminated form) of the final consonant of the monosyllabic root, or (c) a separation of the final and penultimate consonants, both these latter with an intervening *-u-*.<sup>28</sup> This is something of a hybrid type, having—besides the unique feature of root vowel mutation—some features in common with both type (1) and type (2), but having the same peculiar tone-pattern as type (3) and in some words even shapes which could be type (3) plurals, e.g. (a) *gàatarii*, pl. *gaaturàa* ‘axe’, *hakàrkarii*, pl. *hakurkuràa* ‘rib’, *rawàanii*, pl. *rawunàa* ‘turban’ (cf. *riigunàa* in (3)); (b) ‘*àbù*, pl. ‘*abubbàa* ‘thing’, *kàshii*, pl. *kasussàa* ‘bone’; (c) *tafkii*, pl. *tafuk(k)àa* ‘pond, lake’ (cf. *taafukàa* in (3) ), *gamjii*, pl. *gamuzzàa* ‘the tree *Ficus platyphylla*’, *sàuraa* (< \**sàkraa*), pl. *sakur(r)àa* ‘disused farm’. And with these three sub-types one should perhaps include (d) those trisyllables that are identical with sub-type (a) except for the fact that the second vowel is *-u-* in the singular too, e.g. *ràakumii/aa*, pl. *raakumàa* ‘(he/she) camel’, *kùnkuruu*, pl. *kunkuràa* ‘tortoise’, (e) trisyllables of other phonic composition with singulars in *-ii* and plurals in *-aa* which differ from type (1) (a) in having a high-high-low tone-pattern in the plural, e.g. *tàakàlmii*, pl. *taakalmàa* ‘(pair of) shoes, sandals’, *lifidii*, pl. *lifidàa* ‘protective quilting worn by cavalry horse/soldier’, *luudàyii*, pl. *luudayàa* ‘gourd ladle’.

In trying to assess the relative time depth of all these varied types of plural formation, various other factors need to be taken into account, e.g.

(i) The typical plural form of a Hausa noun is trisyllabic. Hence it would appear that where (less commonly) a singular form was also trisyllabic (or quadrisyllabic), simple flexional plurals of types (1) or (4) were from the earliest times preferred to those of types (2) and (3), which would have involved adding a fourth (or fifth) syllable. And this method of pluralization was retained with Arabic and other (even some English) polysyllabic loanwords, e.g. *lifidàa* cited above, ‘*àl’amàarii*, pl. ‘*al’amur(r)àa* ‘thing, affair’, *màjàlisàa*, pl. *màjàlisuu* ‘council (chamber)’, *màkaaniikii*, pl. *màkàaniikai* ‘mechanic’, *takòobii* (Berber), pl. *takub(b)àa* ‘sword’, *kamfànii*, pl. *kamfunàa* ‘company’.

(ii) The very few DISYLLABLES of type (1) are all everyday words that are basic to the vocabulary.

(iii) Plurals of type (2) also include many basic items of vocabulary, especially names of wild animals with level toned singulars and plurals in *-CàaCee*, e.g. hyena, jackal, rat, hare, hyrax, roan antelope, elephant, buffalo, monitor bird (also the older,

the tonally identical quadrisyllabic duals/plurals of the *fikàafikii* type, which have the normal *-aa*-infix.

<sup>28</sup> These last two sub-types are rare in standard Hausa, being mainly confined to western dialects.

indigenous, name for the vulture *kooloo*), fish (generic name and some specific names), as well as the domestic horse, sheep and goat, and a few common trees (e.g. *riimii* and *bauree*),<sup>29</sup> and the generic word for leaf, foliage; also a few basic artifacts, e.g. mortars, pestles, dippers and torque-bangles.

(iv) Most names of artifacts, household utensils and articles of clothing (male and female) have type (3) or (4) plurals, and so do geographical features and things that are parts of other things or the result of human action on other things ('verboid' nouns), e.g. valleys, lakes (though mountains and hills are type (2)),<sup>30</sup> towns, villages, grass, holes, heaps, wounds (*tabòò* 'scar', however, has types (1), (2) and (3) plural forms) and things in general. Parts of the body, and names of weapons and musical instruments are about equally divided between types (2) and (3), but the body itself (alive *jikii*, or dead *gaawaa*) is type (3), as well as the mouth, the nose and the stomach.

(v) Indeed there is a close correlation between the singular form of a number of words of type (3) and VERBAL nouns, e.g. *kwarii* 'valley', *raamii* 'hole', *daakii* 'hut', *hakii* 'grass', *kai* (< *kaayii*) 'head', *gàrii* 'town', *ràunii* 'wound'; cf. *harii* 'raiding', *kaarii* 'increase', *niishii* 'groaning', *ginii* 'building', *rai* (< *raayii*) 'life', *taarii* 'collection', *t(w)àarii* 'cough', *hàkii* 'panting', *zàrgii* 'blame'. Moreover a number of verbal nouns of these forms which have acquired a concrete meaning also operate plural forms of this type, e.g. *yankii*, pl. *yankunàa* 'slice, portion', *kashii*, pl. *kasuusuwàa* 'heap, lot, fraction', *raayukàa* 'lives', *tàafii*, pl. *taafunàa/ukàa* 'hand clapping; palm, sole'.

(vi) There are some basic concepts among the less numerous sub-types (ii) and (iii) of type (3), e.g. grandparents/ancestors, days and months (see above), also some other words of presumably great antiquity like *gòonàkii* 'farms' (cited above), and a couple of names of human or quasi-human animates of presumed antiquity, viz. *mànzoo*, pl. *mànzànnii* 'messenger' (now being superseded by *maasinjàa*) and *dòdoo*, pl. *dòdànnii* 'goblin, ogre, evil spirit'. But the ONLY names of wild or domestic animals having plurals of this type are (surprisingly) 'lion' *zaakli*, orig. pl. (still current in Niger) *zaakunàa*,<sup>31</sup>

<sup>29</sup> Many tree names, however, have no distinct plural form. Examples of such with level tones are *dunduu*, *hanuu*, *kaawoo*, *markee*. Some crop names are in the same class, e.g. *hatsii* 'corn' (genetic term), *geeroo* 'bulrush millet', *baabaa* 'indigo', *gujiyaa*, pl. (het.) *guzàayee* '(Bambara) ground nut'; but the majority of crop names are either heterotonic, or else known or patent loanwords.

<sup>30</sup> So is the older (western) word for river, *gulbii*, pl. *gulàabee*; but the standard word, *kòogii*, pl. *koogunàa*, is type (3). Both refer basically to a river bed or channel.

<sup>31</sup> The standard plural is heteroclit type (2a) *zaakookii*. Unlike most wild animals, it has a feminine form *zaakanyàa* 'lioness'. For similar heteroclit plurals cf. the verbal noun *yaaakii* 'war', pl. *yàake-yàakee*, *yaaakuukuwàa* and (standard) *yaaakookii*, and the Arabic loanword *laifii* 'wrong doing, fault', pl. *laifufukàa* and (standard) *laifoofii*.

'ram' *ràgòò*, pl. *raagunàa*,<sup>32</sup> 'hawk' *shaafòò*, pl. *shaafunàa* and 'ox-pecker bird' *càrkii*, pl. *carkunàa*.<sup>33</sup>

(vii) No adjectival nouns WHATSOEVER have plurals of either type (3) or type (4), though they have them in most of the sub-classes of types (1) and (2), e.g. *kàzaamii*, pl. *kàzàamai* 'dirty', *danyee*, pl. *danyuu* 'unripe, raw', *gàjeeree*, pl. *gàjèeruu* 'short', *bàakoo*, pl. *bàakii* 'strange, foreign', *màkaafòò*, pl. *màkàafii* 'blind', *tsoofoo*, pl. *tswàffii/tsòofàffii* 'old', *bakii*, pl. *bakàakee* 'black', *doogoo*, pl. *doog(w)àayee* 'tall, high, long', *kurmaa*, pl. *kuràamee* 'deaf', *shuudli*, pl. *shuddaa* 'blue', *kaatòò*, pl. *kaàtaa/kàttii* 'physically big', *gurgùu*, pl. *guràaguu* 'lame', *muugùu*, (< \**miigùu*?), pl. *miyàaguu/muugàayee/mùggaa* 'ugly, bad'. Even where a type (3) or (4) plural form is phonologically indicated, they have a heteroclite type (1) or (2) form instead, e.g. *kàramii*, pl. *kàràmai* 'small' (contrast *kàbakii*, pl. *kabukàa* 'large portion of *tuwoo*'), *waawaa*, pl. *waawàayee* 'foolish' (contrast *gaawaa*, pl. *gaawàwwakii* 'corpse'), *gwànii*, pl. *gwanàayee* 'expert' (contrast *kwàrii*, pl. *kwaruuruwàa/kwarurrukàa* 'quiver').

(viii) Neither do any adjectival nouns have plurals of the *-CooCii* sub-type (2), which, as stated above, is clearly a relatively modern, innovationary type, now spreading rapidly through substantives but still leaving adjectives untouched.

(ix) Indeed the total absence of the disyllabic tone-pattern low-high among adjectival nouns, with the sole exceptions of *bàakoo* and *gwànii* (above) seems to me to be very significant. (For *bàbba(a)* 'big, important' see Parsons 1955, 394. I am now, however, inclined to analyse this word as a sensory quality type formation from a uniliteral base *b-*).

#### IV. MORPHOLOGY IN RELATION TO VOCABULARY

Now, by collating these nine points and following up in particular the last one, it would seem to me possible to make a diachronically layered analysis of all present day Hausa nominal vocabulary somewhat as follows:

(1) The oldest stratum, comprising in the case of disyllables all the high-high (i.e. atonic) nouns, and in the case of trisyllables most of the nouns having the tone-pattern low-high-high (i.e. atonic with a less heavily stressed or lowered initial syllable), all of them having, if pluralized, plurals of types (1) or (2), but in the latter case restricted to those with the termination *-ee*. To the disyllables belong (i) all

<sup>32</sup> It is highly probable that this word originally had a plural \**ràdagii* of type (1a) analogously with *zàabii*, *tsàakii*, *bàakii*, *kwàadii* etc., but, so far as I know, this form no longer exists in any dialect.

<sup>33</sup> The dog, *kàree*, whose original singular form *kàrne* is still extant in rustic Kano Hausa, has a number of plural forms. The standard one is type (3) *karnukàa*, but the others, which are somewhat aberrant, viz. *kàrnai*, *kàrnau* and *kàrnàawuu*, may well be older.

the abstract nouns of sensory quality signifying basic concepts of dimension, weight, touch, taste and smell; (ii) many of the names of wild animals and some trees with which the Hausas may be presumed to have been familiar from the earliest times (the lion being a striking exception) including the mosquito (*samroo/sabroo/sauroo*—no pl.); (iii) a few basic utensils made either of wood, e.g. mortars, pestles, canoes, or of clay, e.g. bowls and cooking-pots, plus one or two metal objects, e.g. torque bangles (*munduu/munduwaa*, pl. *mundàayee*); (iv) the names of the elements earth, fire and water, plus the general words *wurii* for place and time, and *wajee* for neighbourhood, surroundings; (v) the basic colour terms, 'black', 'white' and 'red'; (vi) a number of other socially basic adjectival nouns, e.g. 'deaf' (*kurmaa*), 'deaf-mute' (*beebee*), 'fool' (*waawaa*), 'wastrel' (*ragoo*), 'insufficiently clad' (*huntuu*), 'cantankerous' (*huutsuu*), 'stupid' (*waawaa*); (vii) the word for 'mother', *'uwaa*, with its plural *'uwàayee*/*'iyàayee* signifying 'both parents'; (viii) a few parts of the body (rarely pluralized), e.g. *baayaa* 'back', *hanjii* 'entrails', *jinii* 'blood', *kaashii* 'excrement', *toozoo*, pl. *toozàayee* 'hump (of cow or camel)'; (ix) a few weapons, e.g. knives, cudgels and throwing sticks. To the trisyllables belong (i) a number of other basic adjectival nouns, e.g. 'small' (*kàramii*), 'filthy' (*kàzaamii*), 'short' (*gàjee-ree*), including all the verbal adjectival nouns, e.g. 'dead' (*màtacee*), 'broken' (*fàsas-shee*), 'sufficient' (*'isasshee*) and the adjectival nouns of sensory quality, e.g. *kàkkarfaa* 'strong', *fàrfaadaa* 'broad, extensive'; (ii) several names of animals, both wild and domesticated, including specialized names for the male, young etc. of domestic animals, e.g. 'ostrich' (*jìminaa*), 'wild cat', 'tom cat' (*mùzuuruu*), 'tortoise' (*kùnkuruu*), 'humpless cattle' (*mùturuu*), 'squirrel' (*kùreegee*), 'camel' (*ràakumii*), 'he-goat' (*bùnsuruu*), 'calf' (*màrakii*), 'foal' (*dùkushii*), 'heifer' (*kàrsanaa*), 'pullet' (*sàagaraa*) (iii) a number of other miscellaneous words which appear to be basic in the vocabulary, e.g. 'wet season' (*dàamanaa*), 'destructive fire' (*gòobaraa*), 'mat' (*tàabarmaa*), 'axe' (*gàatarii*), 'farmlands of a village' (*kàrkaraa*).

(2) Another old stratum of nouns which differ from (1) in that the disyllables have the tone-pattern either high-low or low-high in the singular, but whose plurals are all of types (1) or (2), like those of stratum 1. These comprise (i) basic natural and sociological divisions of mankind, e.g. male, female, slave, pagan; also the homestead (see end of note 23); (ii) the basic domestic animals, viz. 'horse' (*dookli* < \**daukli*), 'sheep' (original masc. sing. form \**tumkii*) and 'goat' (ditto. \**aukli*), which share a common plural form, *dawaakii*, *tumaakii*, *'awaakii*, 'chicken' (*kàazaa*, pl. *kàajii*), the 'chick' (*tsàakoo*, pl. *tsàakii*) and the 'donkey' (*jàakii*, orig. pl. *jàakai*);<sup>34</sup> (iii) some smaller wild animals, e.g. 'monkey' (*birii*, pl. *birai*), 'guinea-fowl' (*zàaboo*, pl. *zàabii*), 'frog' (*kwàadoo*, pl. *kwàadii*), 'stink-ant' (*gwàanoo*, no pl.), 'locust' (*fàaraa*, pl. *fàarii*), 'termite' (*gàraa*, no pl.) and 'fly' (see end of note 23); (iv) a few names of parts of the body, e.g. 'heart' (*zuuci(yaa)* < \**zuktii*, pl. *zukaàataa*), 'limb' (*gabàa*, pl. *gabàai*).

<sup>34</sup> It now has also an equally common type (3) plural *jaakunàa*.

*gàggàbùu*), 'leg/foot' (*kafàa*, pl. *kafàafuu*), 'finger/toe' (*yaatsàa*, pl. *yaatsuu*), 'eye' ('*idòo*, pl. '*idàanuu*) and 'beard' (*geemùu/geemèe*, pl. *gyamàmaa*); (v) some natural features, e.g. *tudùu*, pl. *tùddai/tùddaa* 'upland, hill', *duutsèe*, pl. *duwàatsuu* 'stone, rock, mountain'. The word for wild animals, 'game' (also 'flesh'), *naamàa*, pl. *naamuu*, would seem to be a loanword from Bantu, but the spear used to kill them, *maashii*, pl. (het.) *maasuu* is an indigenous word in this stratum.

(3) The relative antiquity of nouns which now regularly have plurals in *CooCii* is harder to determine. Those few in which the *-oo-* is a base infix and there is no reduplication, e.g. *riiji-yaa*, pl. *riyoojii* 'well' and *tsarkiyaa*, pl. *tsarookii* 'bowstring', are probably as old as *dawaakii* and *tumaakii*, the *-oo-* being a mere variant of *-aa-*, and the singular tone-patterns (discounting the feminine suffix) being the same in both cases, viz. high-low. But it is not so easy to place those such as *koofàa*, pl. *koofoofii* 'door/gateway', or *baràa*, pl. *baroorii* 'servant', or *buutàa*, pl. *buutoocii* 'gourd bottle', or '*iskàa* 'air, wind', pl. '*iskookii* 'evil spirits', or *taskàa*, pl. *taskookii* 'store-chamber', where there is reduplication of the final consonant. These latter far outnumber the former today, and some of them, such as those cited, would appear from their meanings to be original Hausa words. (But contrast *fuskàa/fuskookii* 'face', which has another, obsolescent plural form *fusàakaa*). Neither is it obvious why this type of plural should have been selected as the predominant one for loanwords from a variety of languages,<sup>35</sup> except that the oldest of these mostly have singulars ending in *-aa*, and disyllables the tone-pattern high-low, these being the two differentiae for indigenous nouns of the class (note feminines in *-iyaa* like *riijiyaa* correspond both to basic masculine forms in *-ii/èe* and to basic feminine forms in *-àa*). Examples are *kàasuwaà*, pl. *kaasuwooyii* 'market' (Arabic via Kanuri) (see Greenberg 1960, 210), *taagàa*, pl. *taagoogii* 'window' (Arabic), *saa'aa*, pl. *saa'oo'ii* 'hour, time' (do.), *sàna'aa*, pl. *sana'oo'ii* 'occupation, trade' (ditto), *mootàa*, pl. *mootoocii* 'motor-car' (Eng.), *direebàa*, pl. *direeboobii* 'driver' (do.), *baasùkùr*, pl. *baasukuroorii* 'bicycle' (do.).<sup>36</sup> The admittance of polysyllables to this type of reduplicated plural alone, however, was probably the result of an earlier process in native vocabulary, whereby feminine singulars which had lost their original masculine counterparts also lost their original plural forms based on the latter, replacing them with feminine plurals of this *-CooCii* type in which the *-waa/yaa* suffix is treated

<sup>35</sup> Perhaps it was simply that Hausas liked the sonority of this form with its medial *-oo-*, which (except for a very few words such as *takòobii* 'sword'—a Tuareg loan: the indigenous word for 'sword' is *kànsakàlìi*, pl. *kansakulàa*) is confined to plural nouns. Curiously enough, however, Yoruba and other Nigerian loanwords generally have type (1) plurals, e.g. '*àgwàagwa* 'duck', pl. '*àgwàagii*; *kaarùwàa* 'prostitute', pl. (het.) *kàarùwai*; '*àngùlu* (Nupe) 'vulture', pl. '*àngùlai*; or else either type (1) or type (3) e.g. '*àgoogoo* (Yor.) 'watch, clock', pl. '*àgòogai* or '*àgoogunàa*, '*àkwàati* (from Nupe *akpoti*) 'box', pl. '*àkwàatai* or '*akwaaturàa/akwaatuttukàa*.

<sup>36</sup> All loan words with a consonantal ending in the singular have plurals of this type, cf. *kaamùs*, pl. *kaamusooshii* 'dictionary'. (*teebùr*, pl. *teeburoorii* or *teeburàa* 'table' is a partial exception).



as though it were part of the original word. Hence, e.g., *zuuciyooyii*, beside the older *zukaàtaa*, as the plural of *zuuciya* 'heart', *tukwàanee* and *tukunyoooyii*, pl. of *tukunya* (< \**tuk(w)n-ya*) 'cooking-pot', *kibau/kibai* and *kibiyooyii*, pl. of *kibiyà* 'arrow', *gàrkii* and *garkuwooyii*, pl. of *gàrkuwaa* (< \**gàrkoo*) 'shield', *gwàddii/gwàddai* and *goodiyooyii*, pl. of *goodiya* 'mare', *riyooyii* and *riijiyooyii*, pl. of *riijiyaa* 'well', *saayuu/sàyyuu* and *saiwooyii*, pl. of *saiwaa* (W.H. only now *saayèe*) 'root'. Indeed many native nouns now have only feminine forms, both in the singular and in the plural, and many of them show by their level tones, as well as by their basic meanings, that they are historically on a par with the level-toned non-suffixed nouns of stratum 1, e.g. *shirwàa*, pl. *shirwooyii* 'kite', *beeguwa*, pl. *beeguwooyii* 'porcupine', *buushiyaa*, pl. *buushiyoooyii* 'hedghehog', *hazbiyaa*, pl. *hazbiyooyii* 'speckled pigeon', *kaaguwa*, pl. *kaaguwooyii* 'crab'.<sup>37</sup> (It is curious however, that the leopard should have a non-feminine name in this class, viz. *dàamisàa*, pl. *daamisooshii*, rather than in one of the older disyllabic classes like most of the other animals with which it is associated in folk-tales).

(4) Of the suffixed plurals (type (3)), those with *-akii* and *-an(n)ii* would appear to be a good deal older than the others. There are comparatively few words in these two classes, but this is accounted for by the fact that the singular forms have a very restricted phonological range, in the case of the *-akii* plurals almost only *CaaCaa*, and in the case of the *-an(n)ii* plurals either *CàaCaa*, or *CàrCaa*, or *CarCaa*, or *CùCaa/ee*.<sup>38</sup> Why these particular patterns of nouns should have been singled out (apparently *ab initio*) for such a special type of plural formation is quite obscure, but most of the nouns in the two classes are basic to the vocabulary and presumably original, e.g. the words cited above, and also *zaanaa*, pl. *zàanàkii/zàanà(i)kuu* 'grass fencing mat', *kaayaa*, pl. *kaayàyyakii* 'load, tool, gear', *bàabaa*, pl. *bàabànnii* 'eunuch', *fartanyaa* (< \**fartaa*), pl. *farèetanii* 'hoe', *gàrmaa*, pl. *garèemanii* 'trenching-hoe', *fàrkaa*, pl. *farèekanii* 'fornication, paramour', *'ùbaa*, pl. *'ùbànnii* 'father', *fùree*, pl. *fùrànnii* '(tobacco) flower', *kùbee*, pl. *kùbànnii* 'sheath'. The fact, however, that the similarly patterned adjectival noun *waawaa* does NOT have a plural of this type, but in stratum 1, and also *jaa* (< \**jaajaa*), pl. *jaajàayee* 'red', would seem to suggest that these words do not belong to quite so old a stratum as the first three.

(5) Finally we come to those nouns, all disyllabic, with tone-pattern high-low or low-high and plurals formed by the suffix *-unàa/ukàa/uwàa*. As stated above, there is a close correspondence between the singular form and the meaning of many of these nouns and those of verbal nouns, especially those in *-ii* or *-li* signifying the

<sup>37</sup> The only exception is *giiwaa*, pl. *gliwàayee* 'elephant', which is a feminine form of the obsolescent *giyèe*.

<sup>38</sup> *Watàa*, *mànzoo* and *dòdoo*, cited above, and *kòogoo* 'cavity (in tree, rock, or tooth)', are anomalous exceptions. (*CooCoo*, however, is merely a W-coloured version of *CaaCaa*: see Parsons, 1970, 278).

result of a verbal action, e.g. *tàafii* 'handclapping', then 'palmful' (and then 'palm, sole'), *mòotsii* 'movement', *yankli* 'slice', *rabli* 'half', *taarli* 'collection (of things)', *ginii* 'building', *tsaunii* 'pile, hillock', *taakii* 'trampling (by cattle of farm), manure', *dafii* 'decoction, poison'. Indeed the difference between the two types lies solely in the fact that the non-verbal nouns also have plural forms, whereas very few of the verbal nouns do, at least not plurals of this nominal type.<sup>39</sup> And it would appear that the former type developed at a later stage of the language on the analogy of the latter, and were then given plural forms of a type that was quite new to the language, possibly borrowed. It is significant, for instance, that theirs is almost the only case where a suffix is allomorphic and the variable consonant is largely determined by the C<sub>2</sub> of the base (not, as is the case with plurals of the older type, mainly by tone-pattern or by radical and/or terminal VOWELS). To this class belong all such words as *raamli* 'hole', *ràunli* 'wound', *tsaanli* 'ladder', *hakli* 'grass, weeds', *kwari* 'valley', *kwàrli* 'quiver' (with neutralized pl. *kwaruuruwàa/kwarurrukàa*), *daajli* 'bush', *yaajli* 'condiment', *gàrli* 'town', *daakli* 'hut, room', *'àbli* 'thing', and many names of parts of the body, e.g. *jikli* 'body', *cikli* 'stomach' (a v.n. of *cik*- 'fill'?), *bàakli* 'mouth', *hancii* 'nose', *gòoshii* 'forehead', *gindii* 'bottom, waist', *kwàurli* 'shin (bone)', *gaashii* 'hair, fur, plumage', *jìbli* 'sweat', *karli* 'sting (of bee or scorpion)', *dàagii* 'paw of feline'. Later the class acquired many nouns with other terminal vowels than *-ii*, including most of the basic names of articles of clothing, male and female, e.g. *riigaa* 'gown', *wàndoo* 'trousers', *bàntee* 'loin-cloth', *zanèe* 'woman's body cloth', *fùulaa* 'cap', *sàacee* 'reinforcement of gown', and the names of many domestic utensils and artifacts, e.g. *bàkaa* 'bow', *tùuluu* 'water-pot', *rùmbuu* 'corn-store', *rùmfaa* 'booth', *ràndaa* 'storage jar', *kèekèe* 'sewing-machine, typewriter, bicycle, cart', *kwàndoo*, *lèefee* 'types of basket', *jàkaa*, *bùhuu*, *san hòo* 'types of bag and sack', *kòokoo* 'small calabash', *làujee* 'sickle', *daagii* 'digging-stick' (whose pl. *daagunàa* neutralizes with *daagunàa* 'paws'). Many of these words are probably loans from other languages with which the Hausas had cultural or economic connections, the name doubtless being introduced with the article (*bàntee* 'loin-cloth' almost certainly is). It is significant at least that all these later words with variable terminal vowels tend to have but one form of the allomorphic suffix in the plural, generally *-unàa/inàa*,<sup>40</sup> not like the older members of the class in *-ii*, which generally have two or more alternants, usually *-uwàa/-ukàa*, and the first named is now the most productive form of the suffix for the modern loanwords in the class. The exceptions are where there is some counter-indication in the phonology of the base, e.g. *kwaanòo* (from Yoruba *kpanu*, from Eng. 'pan') 'head-pan, metal bowl etc.', pl. *kwaanunnukàa*, because the *-n-* of the base precludes the *-unàa/inàa* suffix (also type (1) *kwànnaa* and heteroclit *kwaanoonii*) and *laayli* 'line etc.', pl. (cited above) *laayuyyukàa*,

<sup>39</sup> There is a special and uniform type of plural formation for verbal nouns, e.g. *yànke-yànkee* 'acts of cutting, slaughterings etc.'. A similar dichotomy of course exists with '-ing' words in English.

<sup>40</sup> For this phonologically determined variation in the shape of the suffix (which has not been recognized in grammars of Hausa) see Parsons (1970), 285, n. 41.



probably on the analogy of *raayukàa* 'lives' and *kaayukàa* 'villages'. But contrast, e.g. *tantii*, pl. *tantunàa* (also heteroclit *tantoocii*) 'tent, tarpaulin', *famfòò*, pl. *famfunàa* 'pump, pipe', *bankì*, pl. *bankunàa* '(money) bank'. A few nouns in this class, however, such as *kauyèè*, pl. *kaayukàa* 'village' and *zaurèè*, pl. *zaurukàa* 'ante-chamber', are harder to chronologize, for (i) they would appear to be indigenous words of some antiquity, but (ii) they do not end in *-ii* and theirs is not a verbo-nominal pattern and (iii) their plurals have no allomorphs in this class (*kauyèè*, however, has a dialectal variant *kawàyyaa* of type 2).

Furthermore I think it is of significance that, almost alone of the three main types of plural, the suffixed type (3) embraces disyllabic words of more than one tone-pattern—high-low or low-high with the *-unàa/-uwàa/-ukàa* class, low-high or high-high with the *-a(n)nii* class (only the *-akii* class are all high-high)—whereas with type (2) plurals the different plural classes distinguished by different terminal vowels correlate precisely with singular tone-patterns—high-high with plurals ending in *-ee*, high-low with those ending in *-aa*, *-ii* and *-uu*. In fact the great majority of singulars with the low-high pattern have plurals in one of the suffix classes. This suggests that the low-high tone-pattern may well have been an innovationary one for nouns *pur sang*, and that it came in either by way of verbal nouns (in which the pattern is common with all five vowel terminations and the only one for those in *-au*), or else by way of loans from other African tonal languages which had this pattern in their nouns,<sup>41</sup> or possibly both at once. In corroboration of this theory is the almost complete absence of this tone-pattern among adjectival nouns (which are very rarely cognate with verbs, an exception being *shuudii* 'blue'), to which reference has already been made. Against it, however, are (i) that plurals of type (1)—the 'polar' and 'polar plus' classes, which I have assumed to represent the oldest strata of nouns in the language—have singulars of all three disyllabic tone-patterns, e.g. *danyee*; *baawàa*, *mijii*, *birii*, 'arnèè; *màataa*, *kàazaa*, *bàakoo*; (ii) the plurals of trisyllables, which are nearly all (modern loanwords apart) of the polar or polar plus types, correspond to a variety of tone-patterns in the singular, even among indigenous nouns of presumed great antiquity (though there would appear to be a prescriptive low-high-high pattern for adjectival nouns, both radical and derivative, and quasi-adjectival nouns like *bùnsuruu*, *màrakii* etc.), and, if there was tonal variety among trisyllabic nouns from the earliest times, it is to be presumed that there was equal variety amongst disyllabic nouns.

Led me add that plurals of the hybrid type (4) with internal vowel ablaut and/or low-toned terminal *-àa* have so far baffled me in any attempt to chronologize them relatively. They include a few, presumably old, native words like *màrakii* 'calf', *gàatarii* 'axe', *hàntsakii* 'pincers', *dàmfanii* 'grass fence', *kandàmii* 'pond', but the

<sup>41</sup> A good example is the word 'addaa, pl. 'addunàa 'matchet', where both the implement and the name for it were taken from the Yoruba, the geminate *-dd-* representing an approximation to the wider pitch interval of the Yoruba word.

majority are loanwords, e.g. *cookàlii* 'spoon' (native *cibii* and *kùyaafàa*), *takòobii* 'sword' (Berber), *hankàlii* 'consciousness, sense, mind', *had/tsàrii* 'danger, accident', 'àlkalàmii 'pen' (all Arabic), *ràakumii* 'camel' (Ar.?), 'akùshii 'wooden food-bowl'(?).

The significance of the foregoing attempted diachronic stratification of the nominal lexicon of Hausa, in which through mainly morphological arguments we have been led back to the earlier subject of vocabulary, is twofold in relation to the Chadic hypothesis, viz. (i) the 'Chadicity' of the actual plural morphemes themselves; (ii) the 'Chadicity' of the roots upon which the various morphemes operate. My knowledge of Chadic languages is far too scanty for me to say how far, if at all, (the various plural morphemes of Hausa themselves are comparatively relatable as, e.g., are the prefixes of Bantu languages), or whether some are more 'Chadic' than others—my impression from what I have read about them is that plural morphemes in Chadic languages are too manifold and heterogeneous for any obvious comparisons to be made among them, at least so far as I know none has as yet been attempted. But, as it is on lexical vocabulary, not on morphemes, that the genetic hypothesis is mainly based, it is the singular forms, which are presumed to be in each language the basic forms, we are most interested in. The nouns (including in this category pronouns, adjectives and numerals) in Newman and Ma's comparative list greatly out-number the verbs, comprising 100 out of the 145 items. For present purposes we may discount pronouns and numerals,<sup>42</sup> and also those items glossed as nouns in which the Hausa word cited is either a verb *pur sang*, viz. no. 98 'sore' H. *tande* 'lick, skin', or a verbal noun, viz. no. 3 'big' H. *girmā* v.n. of *girm-* 'grow (big)' basically meaning 'growth', no. 69 'mud (for building)' H. *taḃo* v.n. of *taḃ-* 'touch' (and the Hausa word means 'mud' resulting from contact by feet, e.g. round a well, NOT building mud, which is *kasaa*), and no. 114 H. *ḃimī* v.n. of *ḃim-/ḃum-* 'beat etc.' meaning 'warmth'. Of the remaining nouns, more than half (52 out of 86) have Hausa words cited against them. It is these 52 words we need to examine most closely in the light of what I have said above about Hausa nominal vocabulary. The breakdown is interesting, as follows. Exactly half of the Hausa words, i.e. 26, belong to my stratum 1, i.e. are level toned words with plurals of type (2). They include some very common and basic Hausa words, e.g. *gwankii* 'roan', *ḃaunaa*

<sup>42</sup> It has not, I believe, been remarked before how divergent in form the words in these two minor, but universal, classes are in all African, including Bantu, languages, in contrast to their uniformity in Indo-European languages. The heterogeneity of numerals is not surprising, considering that there are so many different systems of counting current in Africa even today, and many of the numerals are simply transferred terms meaning literally 'hand' or 'fist'. Where there is some apparent homogeneity, e.g. with 'four' in Chadic (no. 38 \**f-d-* in Newman and Ma's list of cognates), one may suspect borrowing to have taken place, as has happened with the Arabic *àshirin* 20, *minyà* 100 and 'àlīf 1000, largely supplanting the native words *haiyaa* (or *lasòo*), *dàrii* and *dubuu* in Hausa. (Indeed some of the lower numerals, e.g. *bakwàì* 7 and *takwàs* 8 are on phonological evidence unlikely to be original Hausa words). The lack of homogeneity in the pronouns, however, is more surprising, and this would seem to suggest the splitting of cognate languages at a much earlier time than in Indo-European.

(< \**baknaa*) 'bushcow', *karaa* 'cornstalk' (though here glossed 'fence'), *turmii* 'mortar', *tukunyaa* 'cooking-pot', *kiifii* 'fish', *tsuntsuu* 'bird', *wurii* 'place', *jinii* 'blood' and the adjectival nouns *farii* 'white' and *jaa* 'red'. In addition there are nine words from my stratum 2, all basic to Hausa vocabulary, viz. *kàazaa* 'hen', *zàkaràa* 'cock', 'àkwiyaà (< \**aukiyaa*) 'goat', *dookii* 'horse', *tunkiyaa* 'sheep', *birii* 'monkey', *zàaboo* 'guinea-fowl', 'idòò 'eye' and *geemùu* 'beard'; only four from stratum 3, viz. *daudàa* 'dirt', *wiyàa* 'neck', *turbàa* 'road, path', and the variable plural noun *kadàa/kadòò* 'crocodile'; only two from stratum 4—but these are very few in number anyway—viz. *kadàa* (again) and *kaayaa* 'load'; but as many as ten from the most recent stratum 5, including *kai* 'head', *kadàa* (again), *kâr(n)ee* 'dog', *hancii* 'nose', *jikii* 'body', *kàshii* 'bone' and *tùuluu* 'water-pot'.

It is difficult to see quite how much value to assign to the morphological argument based on the plurals of these words in Hausa, and their tonicity in the singular forms, especially as one of those in the last stratum, *kàshii*, does seem to be a pan-Afro-Asiatic word, cf. Egypt. *qs*, etc.<sup>43</sup> But it does seem significant that in the Newman and Ma list there are only three words with polar (plus) plurals, viz. *sùrukii* 'in-law', *saiwaa* 'root' (feminative form of *saayèe*, pl. *saayuu*) and *kibiyàa*, feminat. sing. of *kibau/kibai* 'arrows'.<sup>44</sup> There is not a single example of the sixty odd abstract nouns of sensory quality, nor of such words as 'man', 'woman', 'father', 'mother', which are the most basic items of common Indo-European vocabulary, nor any tree or crop name. For 'slave' there is no cognate series with Hausa *baawàa*, pl. *baayii*, but a quite different root \**d-b-* with examples from only two Chadic languages, and the same applies to 'sun, day', 'moon' and 'night'. The list, furthermore, is totally lacking in adjectives, except for 'red', 'white' (and this is in the 'second level confidence' list at the end) and 'big' referred to above, plus 'bitter', 'clean', 'fat', 'good' and 'many', which have no citations from Hausa. But this is perhaps not so surprising, since even basic adjectives vary more than substantives among related languages, since they are emotive rather than deictic words.

Admittedly my whole theory of chronologizing Hausa nouns on the basis of (a) their plural forms, and (b) their meanings is open to many criticisms, of which the strongest is that there is no documentary evidence to support it.<sup>45</sup> Also that it

<sup>43</sup> There are, however,—as with *kàree*—other, and presumably older, plural forms of this word still extant in dialectal Hausa, viz. *Rasussàa* (type 4), *Kašsaa* (type 2, but heteroclite) and *Ràssai* (type (1b)).

<sup>44</sup> Remarkably, however, under no. 49 'hoof' *xk-p-*, the Hausa word *kòofatòò*, pl. *kòofàtai*, is not cited, though it is almost identical with the Tera word *kopoto*. Perhaps they considered the former to be a loan from the latter.

<sup>45</sup> The evidence of oral literature, as recorded e.g. in Edgar's *Tatsuniyoyi*, might have some negative value. But it cannot be relied upon entirely, since successive transmitters of stories are likely to have introduced or substituted 'modern' words to increase their appeal to successive audiences. Certainly proverbs and Wellerisms are no guide to the relative time depth of Hausa vocabulary, for these are both still very much a living art form in Hausa, as shown by the number that contain modern loan-words, the supreme example being the very popular Wellerism *Shaakulaatìn Bàngarò ùngùlu taa*

conjugates up a naïve and simplistic picture of the 'primitive' Hausas and the things that they were accustomed to and needed to name. Why, it may be objected, should they not have kept dogs (as well as horses, sheep, goats and chickens), hunted lions and leopards (as well as birds, hares, fish, bushcows and various antelopes) and worn gowns and trousers, or at least leather aprons (*warkunàa*) (not just *faatuu* 'skins') from the earliest times? Still more have known lakes and valleys (as well as mountains and hills), used spears, hoes and sickles (as well as knives, cudgels and throwing sticks), lived in 'towns' and 'villages' (not just in isolated homesteads), and had names for pits, wounds and 'things' in general? Certainly they possessed grandparents or ancestors, as well as parents, parents-in-law—and, on my theory, slaves. But let us consider the linguistic implications of the OPPOSITE theory, namely that ALL the nouns I have cited (except for the obvious loanwords) are equally old and well-established in the language. In that case, unless—which is extremely unlikely in so obviously conservative a language as Hausa—a great many nouns have lost their original plural forms and substituted a more 'modern' (possibly a borrowed) type, we are left with the most heterogeneous and remarkable 'ur-synchronic' system of conveying identical information, to wit the notion of plurality.<sup>46</sup> Yet it remains a

*ga gaawar mootàa* 'That's not my pidgin (lit. indifference of the butcher), (as) the vulture (said when) she saw the carcass of a motor-car', which contains only two indigenous Hausa words, *ga(nii)* 'see' and *gaawaa* 'carcass', the others being—in order—of obscure origin, Fula, Nupe and English. Nevertheless, the high frequency of certain animal names in proverbs and fixed expressions, e.g. the hyena (*kuuraa*), the monitor (*damoo*) and the rat (*beeraa*) is some evidence for the antiquity of these words.

<sup>46</sup> Far more heterogeneous than are, e.g., the different declensional systems of the oldest recorded Indo-European languages, although there are some parallels between simple flexional forms and suffixed or agglutinated forms (e.g. the *-bus* and *-rum* of Latin declensions) in both. The sheer number, variety and alternativity of Hausa plural forms led one scholar, Taylor (1959, chap. 15) to postulate a theory of semantic dichotomy, viz. 'plurals of paucity' and 'plurals of abundance'. But I have found no substantiation whatsoever for such a theory; moreover it is vitiated by the many forms of words he cites in support that are totally unattested in the dictionaries or by any other Hausa scholar I know. The most one can say is that, where there is a choice between more than one standard form of plural, and it is desired to convey the suggestion of great number (and/or variety), a good writer will choose the longest form: e.g. (from a Hausa version of the Pied Piper story) *Ya ceè sù lilliiikè raammumukàn dà beràayee sukà gagginaa* 'He told them to stop up and seal (one by one) the innumerable holes that the rats had (severally and successively) dug'. Furthermore 'double plurals' (cf. *child-r-en*) are common, e.g. *maatàayee/maatàttakii/màatàakuu* 'women', *'itaatuwàa* 'trees', *shaanànnakii* 'cattle', *dàwàakai* 'horses'; and sometimes these longer forms have a different connotation, e.g. *mazaa* 'men, males, husbands', *mazàajee* 'he-men, heroes'.

Indeed two of the words I have cited above as singulars are in origin plural forms, viz. *hakàrkarii* 'rib' and *hàkoorii* 'tooth'. The former in fact is a morphologically slightly anomalous dual form in the 'pairs and pieces' class (cf. *fikàafikii* 'pair of wings'), as its meaning too would suggest, whilst the latter is a root-disjointed type (2) (a) plural of *haurèe*, a word which in western Hausa is still used to refer to a single tooth, but in standard Hausa is confined to the meaning 'elephant's tusk, ivory'. (Compare too what was said in note 35. Admittedly the derivation of *haurèe* from *\*hakrèe* gives rise to phonological problems concerning the possibility of glottal /k/ occurring as a coda

system of sorts, in so far as it displays a clear tie-up between singular and plural forms at the morphological and phonological levels, and, in the case of disyllables at least, also at the tonal level. But this is not all there is to it. As I have tried to show, there is also a tie-up with MEANING, at least as strong as there is in class languages like the Bantu languages and Fula. We can in Hausa with equal justification talk about 'animate' and 'inanimate' classes of noun, and in the case of Hausa, also of 'substantival', 'adjectival' and 'verbal', or 'verboid', classes. It is only the EXPONENTS of this primitive categorization that differ—not a neat, homomorphic set of prefixes, as in Bantu, or of suffixes, as in Fula, but a combination of tone-pattern with various other morphological features in the singular, and all the various exponents I have described in the plural. And the (I would say later) super-imposition on this nominal system of a two-way concord system of gender which cuts right across it (there are feminine nouns in ALL the plural classes but two) seems to me no more relevant thereto than are tone-patterns to class divisions in Bantu languages.<sup>47</sup>

The implications of all this are obvious. If Hausa is to be classed as a 'class language' (albeit of an unusual type), and Hausa is a Chadic language, then presumably all the other Chadic languages must be—or must have been—class languages too, since 'class' is universally recognized as being one of the most primitive features of language. Let Chadacists, I would say, start at once to look for their classes, so that they may be compared with Hausa and other languages both as to form and content.

## V. CONCLUSION

We have only here discussed in some depth the two most obvious and striking aspects of Hausa morphology, viz. verbal grade and nominal plural forms. But

consonant). *Hakurkuràa* and *hakðoraa*, which were cited as the plural forms above (of types (4) and (1) (a) respectively) are therefore really double plurals; and so is the alternative form *hàkàrkàrai* on the 'pairs and pieces' model (cf. *fùkàafùkai*), ALL the pairs and pieces 'plural' forms being in fact regular type (1) (b) plurals of the 'dual' forms. Indeed, with names of paired or multiple parts of the body in general the reference—in terms of number—is imprecise, even where the word has only two forms, one clearly singular and the other clearly plural. And this imprecision in usage may cut both ways, even to the extent of the plural form being used where the referent is unambiguously singular, e.g. (*Magana Jari Ce*, iii, 229) '*tafasasshen ruwa ya zuba masa a kumatunsa na hagun*'—NOT *kuncinsa na hagun*—'some boiling water got spilt on his left cheek(s)'.

<sup>47</sup> I confess that I have not read enough of the literature on the subject to know whether the masculine/feminine dichotomy of singular nouns is a universal phenomenon in Afro-Asiatic languages, and whether it is presumed to have been an original feature therein—as tone in Bantu languages. In Hausa, at least, it is certainly much less pervasive and canonical than many other features of the language. The speech of Kano is rightly regarded as the norm, but, as one moves away from this, its geographical, centre to the periphery of the monoglot Hausa area, one finds feminine concords progressively more disregarded: there are monoglot speakers in Bauchi who do not even use them with human female referents. Moreover, it is probable that not all the different exponents of gender are of equal antiquity, as has been so ably argued in Skinner 1970.

there are many other things one could adduce for a comparative discussion, e.g. the total absence of any flexional forms of the verb correlating with tense or aspect (contrast the persistence of such forms in all I.-E. languages);<sup>48</sup> the wealth of verbal derivative forms of all sorts, including the verbal adjectival form with its unique combination of both reduplication and gemination,<sup>49</sup> e.g. *màt-a-cc-ee* 'dead', *'is-a-sshe-ee* 'sufficient', *shimfid-a-dif-ee* 'spread out', the augmentative adjectival form, e.g. *ribd-ee-d-èè* 'hulking, huge', *shimfid-ee-d-èè* 'extensive',<sup>50</sup> and the syntactically and semantically interesting class of word I designate 'verbal adverbial nouns of state',<sup>51</sup> e.g. *zàun-e* 'seated', *tàar-e* 'assembled, together', *bùg-e* 'drunken state', *màc-e* 'dead (state)'; also the very regular deverbative nouns of agent, place and implement with prefix *ma-*;<sup>52</sup> and some minor and sporadic forms of nominal flexion and/or derivation too, such as the heteromorphic adverbial noun forms paralleling concrete noun forms,<sup>53</sup> e.g. *bàakii* and *bakà*, *daakii* and *dakà*, *jikii* and *jikà*, *'idòò* and *'ido*, *wajee* and *wàje*, *cikii* and *cikii*, and the little homomorphic class of 'nouns of affinity' like *tsàkà-tsakà*, *jìnà-jinà*, *bàngà-bangà*, *zàngà-zangà*,<sup>54</sup> these last two small classes showing clearly that neither the terminal vowel, nor the tone-pattern is an essential etymological part of the Hausa noun any more than it is of the Hausa verb, but both are primarily grammatical and only secondarily lexical features. One very much wonders what parallels, if any, there are to all these things in other Chadic languages.

To sum up the gist of this paper then, if we take all the lexical and morphological evidence into account, and apply the same sort of criteria as are applied to the classification of English, namely the relative antiquity of items of vocabulary and features of morphology—paying especial regard to morpho-phonology, tonology and what I have called phono-semantics—the relationship between Hausa and the

<sup>48</sup> It is of course debatable how far the Hausa tense system (despite its disjunctive orthography) is to be regarded as basically pre-flexional. But Chadic languages in general have post-flexional, or internally flexional, tense forms.

<sup>49</sup> Indeed this is the only form of word in Hausa in which consonantal gemination appears to be an original grammatical feature in all dialects.

<sup>50</sup> This form, however, is equally derived from ideophones and even concrete nouns. There are many more examples in current Hausa than are cited in the dictionaries. In some cases the bases are unique to this class of word, cf. such slang words as 'scrumptious' in English.

<sup>51</sup> For a brief description of this class see Abraham (1934), 156-158. The existence of such a class of word correlates semantically with the general absence of stative verbs in Hausa, a phenomenon which may well be shared by Chadic, and indeed many African languages.

<sup>52</sup> This *ma-* prefix is also extensively used in Bade, not only in deverbative nouns of agent (of two distinct types), place and instrument, but also to form ordinal numerals and some ethnic nouns, the last being formed in Hausa with a prefix *bà-* (e.g. *Mà-p'n-a-n* = *Bà-haush-èè* 'a Hausa man'): see R. Lukas (1968) 112-116. But it is not clear whether this is a case of borrowing, or of a cognate morpheme. I do not think this *ma-* has any connection with the Arabic *ma-* of place.

<sup>53</sup> Abraham misnames these by-forms 'locatives': they are used in several cases. For further information about them, see Parsons (1961), 115-119.

<sup>54</sup> For a brief description of the class see Parsons (1955), 402. See Arnott (1961), 125-138.



geographically neighbouring Chadic languages appears to be genetically no closer than that which obtains between English and its geographically neighbouring Romance languages. I believe some West Africanists, such as Karl Hoffman, are already classing Hausa as a very 'atypical' Chadic language. It is my conviction that it is so atypical as not to be a Chadic language at all.

One final reflection, however, on the whole subject of genetic relations and classification of African languages. The Bantu field apart—and possibly the Cushitic—, there are no areas in Africa where ENOUGH languages of a postulated family, group, or sub-group have as yet been studied in sufficient depth to establish really convincing criteria of relationship. Not only do we need far more words from far more languages, accurately and scientifically classified at all levels—phonological, tonological, morphological, syntactic and semantic: but we also need to know what words these languages do NOT possess, i.e. the conceptual range of their vocabularies. The task in the case of unwritten and largely undocumented languages is enormous; but, when a score or more of other Chadic languages have been studied to the same depth and extent as Hausa, then we MAY well end up with a list, not just of 150 cognates, but of 1500, or even the 2200 of Bantu!

Personally, however, I am bound to say that I find myself out of sympathy with the whole obsessive urge that is so particularly strong amongst American linguists of the Greenberg school towards a monolithic genetic classification of the vast *mélange* of languages spoken on the continent of Africa today, with each one neatly labelled and pigeon-holed and represented in one-stem branching trees. One realises that this is not done simply for its own taxonomic sake, but for its implications in prehistory and ethnography. But why, one asks, this addiction to one-stem trees and aversion to the acceptance of 'Mischsprachen'? In the natural world, after all, most creatures have more than one parent, so why should not languages too? Nor are genetic classifications the only, or necessarily the most significant, classifications of languages. Typological classifications seem to me to be equally valid, and to have psycho-linguistic and socio-linguistic implications as important as the historical ones of genetic classifications. Thus it is of great significance (if only on the practical plane of mutual language acquisition) that two genetically unrelated languages such as Hausa and Fula should exhibit such a close parallelism in the functions of their tense systems—in particular in the almost identical uses of the subjunctive in the two languages (see Arnott 1961)—or that Hausa and Temne should both have an indefinite pronominal element *a*, or that so many African languages should employ a different lexeme to signify the simultaneous projection of a number of objects from the one they use for a single object, or differentiate between taking one book from a shelf and taking two or three. Even for purely historical research, too, there are many features of language which are rarely mentioned by comparativists but seem to me to be of high significance, e.g. that, alone of all the wild animals to be found in their present habitat, the Hausas have no word for the giraffe, but call it a 'bush camel', whilst many Nigerian languages have no word for the crocodile,

but call it a 'water leopard'.<sup>55</sup> Finally let us remember that individual languages do not operate, and never have operated, in a vacuum. Borrowing and cross-fertilization and acculturation has been going on all the time. If the cult of 'Clockwork Orange' were to gain sufficient momentum among the younger generation in this country, who knows but by the year 2000 Englishmen may not be speaking half in Russian!

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<sup>55</sup> On the other hand of course this may simply demonstrate how childish nicknames for creatures may in time become their commonest, or even their one and only designation: cf. *hedgehog* in English and the Hausa *muɣuɗɗa dawaa* (lit. 'ugly fellow of the bush') = dialectal *gàduu* and *gyaaddo* 'wart-hog', and *bàbba dà jàkaa* (lit. 'big fellow with a pouch') = *boorin tinkèe* (lit. meaning obscure) 'the marabou stork'. Conversely, with certain animals that figure prominently in popular folklore, or have become clan fetishes, the principle of 'avoidance' may have operated and the true, original name for the animal become completely lost. This may well have been the case with the 'king of the bush' in Hausa, i.e. the original word meaning 'lion' has disappeared from the language, being replaced by the morphologically eccentric word *zaakli* (see above *passim*), originally a mere honorific appellation. One can compare *macijiji*, lit. 'biter', which is now the only generic term for a snake, and the many different words for 'hyena' in the dialects of Fula. *Dàamisàa*, too, 'leopard'—a common predator of village flocks and herds—may be an appellation formed from the verb *dàamaa* 'harass, worry' (see note 23).



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## DISCUSSION

JUNGRAITHMAYR: I think Mr. Parsons' paper is so complex and so rich that it would be impossible to discuss it fully here. I would like, however, to go into one question that seems to me to be the most provocative one in his paper, namely whether Hausa can continue to be considered a Chado-Hamitic or Chadic language. I would like to ask Mr. Parsons what exactly were the arguments which led him to propose to us this decision. As far as I can see, he underlines the fact that Hausa, in comparison with the other Chadic languages, is so rich and complex that in his opinion it cannot be compared and cannot be linked with the so-called Chado-Hamitic or Chadic languages. But I wonder whether he has taken into consideration all the relevant research concerning aspects of grammar and vocabulary and questions of sound correspondence and so on that has been carried out for Hausa and those other languages considered so far as having the same genetic origin.

PRASSE: I should just like to raise one minor point concerning the internal plural in Hausa and the Chadic languages. If I understand Mr. Parsons and the Hausa

grammars correctly, what Hausa does is simply to add the vowel *a* to the singular word to give the plural without changing anything apart from that. Well, as far as I know, in the other Hamito-Semitic languages such a phenomenon is only known in Cushitic. I do not think that one could say that the internal plurals of Semitic and of Berber are formed in the same way. It is true that they also include a long *a* but apart from that the rest of the vocalization is also changed. You get a quite new vocalization in the plural.

SKINNER: There is one small question which Mr. Parsons raises to which I have a possible answer, namely where he is speculating where Hausa might have acquired its glottal *k* and its glottal *ts* from. There is at least one clear case in modern Hausa where a word has been borrowed from Arabic, and where a glottal stop coming between *s* and *l*, has merged with the *s* to give a glottalized *ts* in Hausa, namely *matsalaa* 'problem, question, matter' from Arabic *mas'ala* (from the root *s'l*) with the same meaning. So here you have an actual proven case where two phonemes in the loan language have gone to a single glottalized phoneme in the borrowing language. I think I saw somewhere a paper by Professor Hodge where he suggested that possibly the origin of many of these glottalized phonemes was two phonemes that have merged into one. The other one is the *k* in Hausa, which I think Newman and Ma had some difficulty in their paper in saying where it might have come from. Again you do get dialect variations in Hausa itself, for example, the word *kaka* which means interrogative 'how?' is in some dialects 'a'a with simply the glottal stop and although I cannot list them here I have come across a number of what appear to be similarities with related languages—I think particularly Berber—where certain words beginning with a vowel *a*- appear to correspond to Hausa words beginning with *ka* or *ka*. Again, in Hausa the glottal on the *k* seems to come and go fairly freely with some words; for example, the word borrowed from Arabic for 'judge' which in Hausa has gone to *alkali* has also gone to *alkali* with an ordinary *k*. So the glottal appears to be a feature that readily comes and goes in some words.

CASTELLINO: Regarding the connections of Hausa with the other Hamito-Semitic languages, I would like to draw attention to the similarity of the personal pronouns in Hausa and Akkadian. Akkadian is the most eastern and the earliest documented of the Semitic languages, and this similarity between two languages that are geographically so widely separated is striking. But geographical linguistics has shown us that a form may be found on the extreme boundaries and not be present in the central area because the fringe areas are sometimes more conservative than the central ones. I can demonstrate the phenomenon from Latin. If for instance we compare the Italian vocabulary with that of Latin we find that in certain instances Italian has replaced the Latin word, but we sometimes find these words that have disappeared from Italian in Roumanian at the one extreme and in Portuguese at the other. These two widely separated areas have preserved something of the voca-

bulary and also of the phonetics, the phonology let us say, of Latin. And of course the explanation is that the movement has started from the centre and has spread outwards and has not always reached the isolated fringe areas.

ULLENDORFF: I do want to argue vigorously with Professor Castellino. Surely what he has just said is a travesty of the position. If there is a genuine relationship—and relationship surely at the end of the day must mean genetic relationship if it means anything at all—this must mean genetic identity at some stage in the distant past. If he means that this could be postulated in the circumstances of the time scale for Hausa and Akkadian—but then I am sure he cannot possibly mean that—there can be no question here of linguistic geography and propagation towards the extremes from a centre—because there is no centre here from which this could have been propagated towards the extremes. So I do not believe that any phenomenon that is attested in Akkadian could IN THIS WAY, that is in terms of linguistic geography, be found in Hausa in any meaningful way. I do not mean that there are not phenomena which are similar in the two languages, there may be very many phenomena which are similar in the two languages, but they could have no genetic relationship and no relationship which is explicable in terms of linguistic geography.

CARNOCHAN: May I just say that in Bachama there is another glottalized consonant, 'w, as for example in the word 'wayə 'elephant', so that as Newman has suggested we have other forms in addition to *β* and *d'* and 'y in other Chadic languages which may be compared with the glottal *k* or the glottal *ts* in Hausa.

PARSONS: With regard to Professor Castellino's point, I have always understood that the Hausa pronouns, at least one of them, is identical with Arabic *ya-* in *yaktabu*, and Hausa was put into the Hamito-Semitic family on the strength of that alone. Of course there are the two third persons and it is only in the two past tenses that the Arabic (presumably the later form) is standard in all dialects.

Regarding Mr. Skinner's comments, there is another word where you get this anomalous double glottal, namely *daba*, from the Arabic root 'to print', *tb'*. Here the *ba'* and the 'ain have coalesced to give a glottal *β*, but in fact *daβia* with an ordinary *d* is a commoner form. I have argued the whole question of the glottal *k* and the glottal *ts* at considerable length in my forthcoming article.

Now, to answer Dr. Jungraithmayr, I think he has rather missed my point if I may say so. It was not the complexity of Hausa I was going on at all, I merely wanted to point out how much loaning there was, and therefore how dangerous genetic assumptions were as regards items of vocabulary. But my main points were this enormous corpus of vocabulary which seems to be unique to Hausa, and which I have not seen in any Chadic lists and therefore feel needs explanation. The other point is the uniquely simple system of verb gradation which is just like really declining a Latin noun. It seems to me that such a remarkable feature of the language

puts it in a sense out on a limb and one does not know how it could have acquired it. It is almost, I feel sometimes, as if Hausa were a sort of Esperanto.

I am afraid that Mr. Prasse was a little over-simplistic about Hausa plurals—they are positively Homeric in their complexity. It is a complete contrast to the verb—you have infix types with or without reduplication, suffix types with or without reduplication, and in certain presumably old words like *mijii*, pl. *mazaa*, *gijii*, pl. *gidaa* you obtain the plural forms with a simple Latin-type change of ending combined with change of tone. So there is clearly a stratification in time as well as in morphology in the Hausa plurals. I think that it is significant that, just as there are no adjectives in the fourth and fifth declension in Latin, there are no adjectives in certain of the what I would say are possibly later, more Semitic-type *-ukaa/-unaa* suffix plurals.

## BACHAMA AND CHADIC

JACK CARNOCHAN

Using phonological, lexical and grammatical (particularly gender) criteria, Lukas (1936), divided Chad languages into two groups which he called the Chado-Hamitic group and the Mandara group. In 1952, in the *Handbook of African Languages* (Westermann and Bryan 1952), he maintained and amplified this division, renaming the Mandara group "The Chadic Languages, Larger Unit?" (Section IX), but keeping "The Chado-Hamitic Languages (Larger Unit)" for the others (Section X).<sup>1</sup> Between these dates, J. H. Greenberg (1950, 1963), put both groups, together with some Jos Plateau languages, into one branch which he termed Chad, of his Afro-asiatic family. He did not present lexical items and grammatical similarities common and peculiar to the Chad languages, but Paul Newman and Roxana Ma (1966) have in part repaired this omission, in an attempt "to demonstrate conclusively that the Chad family as postulated by Greenberg does indeed constitute a valid linguistic unit". Greenberg reclassified the languages into nine sub-groups, and Newman amalgamated this grouping in general with Lukas' former dichotomy, producing a Biu-Mandara group, which combines Greenberg's groups 3 and 6, and a Plateau-Sahel group, which combines Greenberg's groups 1 and 9. Concerning the languages in Greenberg's groups 2, 4, 5, 7 and 8, examples were not so carefully analyzed, but were included in the comparison rather as corroborative material. Although Newman and Ma were unable in this article to present evidence for Proto-Chadic vowels and tones, their attempt is important at this stage of our knowledge in providing a considerable body of evidence for the setting up of sixteen Proto-Chadic consonants. Their Biu-Mandara group comprises Tera, Bata, Higi, Bura and Wandala clusters. The Bata cluster comprises Bachama, Bata, Cheke, Gudu, Nzangi, Sukur and Zumu languages. I have been working on Bachama, and to a less extent on Bata, since making a field visit to the area in 1963, and propose in this paper to add some evidence supporting Newman's classification of these within the Biu-Mandara group.

I spent most of my time at Numan, working with Bachama speakers, but I also

<sup>1</sup> See Newman and Ma (1966), 218, note 4.

stayed at Song, and made brief visits to the Bata villages Bolki, Bongo, Mudungo, Muleŋ, Jalingo Maiha (at its new site not far from Pakka), as well as Bagale, Njo-bolio, Old Demsa, New Demsa and Farei. It seemed abundantly clear to me that the differences in the speech of all these places were differences of dialect, and I agree with Meek (1931) that "the Bachama and Bata speak the same language". This could mean a consequent modification in the number of 'languages' in the Bata cluster, but I cannot pronounce for other Bata villages in Nigeria and in the Cameroon Republic. I did not go to Gudu, but Bata speakers assured me that the people there do not speak a Bata dialect, and the remark in the *Handbook* (Westermann and Bryan 1952, 160) on the presence of lateral sounds, by which Lukas meant lateral fricative sounds, in Gudu may suggest its removal from the Bata cluster, possibly to the Tera cluster.

Before proceeding to my main theme, may I be permitted to make some constructive criticisms of those sections of Part II of the *Handbook* which deal with Chad languages. This valuable book is out of print, and I understand that it is to be re-issued by a photographic process, which makes alterations in the light of the fresh information available since 1952 just not possible. Since the notations are sometimes phonetic, and sometimes phonemic (but not strictly speaking phonological), my remarks will be of the same order. The Bachama and Bata dialects include among their phonemes

- (1) *p*, a voiceless bilabial plosive,
- (2) *b*, a voiced bilabial plosive,
- (3) *ɓ*, a glottalized bilabial plosive or, in Ladefoged's terms (1964), a voiced laryngealised bilabial plosive <sup>ɓ</sup>*b*,
- (4) *kp*, a voiceless labial velar plosive,
- (5) *gb*, a voiced labial velar plosive.

The first three may be palatalized, *py-*, *ɓy-* and *gy-*, and they may be labialized, *pw-*, *bw-* and *ɓw-*. On page 155 we read "BACHAMA, call themselves *bacama* or *gboare*"; in fact, they call themselves *ɓwa:ɛ* (low-mid tones), which is the plural of *ɓwa:ɾa* (low-mid) masculine and *ɓwa:ɾató* (low-low-mid) feminine. The word *bacama* (mid-high-high) is the name they give to the town shown on maps as Lamurde, the normal home of their chief, whose title is *hama bacama* (mid-mid-mid-high-high) 'Chief of Bachama' (and not 'Chief of THE Bachama'). They refer to their speech as *kwa: ɓwa:ɛ* (mid-low-mid) 'mouth of people'. The Bata refer to themselves as *ɓwa:tiye* (low-mid-mid), and to the Bachama either as *ji-tiŋnótó* (mid-high-low-mid) 'people of Tingno' (one of the most western Bachama villages), or more rarely as *mi-basanjiyan* (mid-low-mid-low-high) 'people of the lower land', while the Bachama refer to the Bata as *mi-basapwa* (mid-low-mid-high) 'the upper people'.

The Bata and Bachama dialects would appear to differ from most Chadic and Chado-Hamitic languages in having no lateral fricatives, and also in having labial velar (Lukas' labiovelar) sounds, according to Linguistic Note (a), page 160.

Note 3 on page 160 says that there are no noun classes in Chadic languages, while note 1 on page 169 says that Chado-Hamitic languages are different in having a two-class or two-gender system in the noun. In note 8 on page 171, Hausa is said to have two singular classes of nouns, and a third class, (plural class). The Bata and Bacama dialects are exactly like Hausa in this respect. If, therefore, the noun class distinction between Chadic and Chado-Hamitic cannot be supported, the differences are weakened and the likenesses strengthened perhaps to the point of preferring Greenberg's unity to Lukas' and Newman's dichotomy.

On page 170, Hausa is said to have three implosive sounds, *ɓ*, *ɗ* and *ʔ* (rare). It is perhaps preferable to regard them as three glottalized (or laryngealized) sounds. Of the sixty-one languages for which Ladefoged gives consonant contrasts, only three, Igbo (No. 67), Kalabari (No. 72) and Kambari (No. 80) have implosive sounds. Writers—myself among them—have been careless in using 'implosive' to describe any sound which has an ingressive air stream mechanism, whereas it should properly be reserved for those plosives which have only one closure apart from the lungs. The vocal cords vibrate during the stop period, and what slight rarefaction is achieved comes from the lowering of the larynx. There are no voiceless implosives, just as there are no voiced ejectives. In the case of glottalized sounds, there is considerable tension of the vocal cords, often producing 'creaky voice'. Ladefoged says, "the vocal cords no longer vibrate as a whole. The ligamental and arytenoid parts of the vocal cords vibrate separately, sometimes almost exactly 180 degrees out of phase with one another, one end opening as the other is closing". In Hausa, *ɓ*, *ɗ*, and *ʔ* are often pronounced without voice at all, and regularly so when the articulation is prolonged as in the intensive form of the verb with corresponding doubling of the letter in the spelling, as in *ɓaɓɓazge*, *ɗaɗɗafa*, etc. Such articulations can scarcely be called voiceless, as the vocal cords are closed, and not held open, and it is better perhaps to refer to them as without voice. Lowering of the larynx may accompany the glottalized plosive, but is not essential to it as it is to implosives.

It is perhaps unfortunate that the post-alveolar glottalized plosive in Hausa is written with 'a kind of *ɗ*', namely *ɗ*; and this goes for other languages too. Apart from the presence or absence of glottalization, the tongue gestures are different; for *ɗ* the body of the tongue is comparatively horizontal, and the tip makes a complete contact with the teeth ridge, the mouth is fairly closed, with the lower teeth fairly close to the upper teeth. For *ɗ* the mouth is more open, the teeth are further apart, and the tongue is more vertical in the mouth, making a retroflex gesture, curled back and then flapped against the roof of the mouth well behind the teeth ridge. This articulation and the place of articulation, are the same as for the flapped *ɗ* in Hausa and in Bachama, and the articulatory similarity between them may not be without significance in comparative studies.

On the basis of the correspondences which they found between languages in their Plateau-Sahel group and their Biu-Mandara group, Newman and Ma reconstructed sixteen Proto-Chadic consonants, arranged like this:

<i>p</i>	<i>t</i>	<i>k</i>
<i>b</i>	<i>d</i>	<i>g</i>
<i>ɓ</i>	<i>ɗ</i>	
<i>f</i>	<i>s</i>	
	<i>z</i>	
<i>m</i>	<i>n</i>	
<i>w</i>	<i>r</i>	
	<i>l</i>	

They also use *D*, *G* and *Z* to denote obstruents with indeterminate voicing, *F* to denote uncertainty between *p* and *f*, and *N* to denote a nasal with point of articulation indeterminate or neutralized. They have reconstructed Proto-Chadic consonants for 144 items, giving English glosses, and using examples from 18 Plateau-Sahel languages, and from 21 Biu-Mandara languages. They regard 119 of these items as their 'first level confidence' list, and the remaining 25 as their 'second level confidence' list. With the help of Mr. E. B. Nadah, a Bachama speaker, I have been through their lists, and am presenting the results to this meeting, but in a very tentative fashion. The Newman and Ma glosses are in column 1, Bachama reflexes which may be cognates in 2, Newman and Ma reconstructions in 3, and Bachama reflexes which do not appear to be cognates in 4. The vowel symbol *ó* represents a central quality, a little more open than half-close, and *ú* an unrounded central vowel with a fairly close quality. The ' represents a glottal stop, or an accompanying glottal stop or glottal reinforcement.

Column 1	Column 2	Column 3	Column 4
<i>Gloss</i>	<i>Bachama Cognate</i>	<i>Proto-Chadic</i>	<i>B. Non-cognate</i>
(1) 'antelope'	—	* <i>g-m-k-</i>	<i>bargiyey</i>
(1a) 'bark (of tree)'	—	* <i>ɓ-r-</i>	<i>pwi:yey</i>
(2) 'beard'	—	* <i>g-m-</i>	<i>mbúsum</i>
(3) 'big'	—	* <i>g-r-</i>	m. <i>kpaney</i> , f. <i>kpantó</i> , pl. <i>kpanye</i>
(4) 'blood <sub>1</sub> '	<i>zambe</i>	* <i>d-N</i>	—
(5) 'blood <sub>2</sub> '	—	* <i>b-r-</i>	—
(6) 'to blow'	<i>fudó</i>	* <i>F-</i>	—
(7) 'to break <sub>1</sub> '	<i>biyó</i>	* <i>ɓ-( )l-</i>	—
(8) 'to break <sub>2</sub> , shatter'	<i>fúló</i>	* <i>p-s-</i>	—
(9) 'breast'	—	* <i>w-d-</i>	<i>duptó</i>
(10) 'buffalo'	—	* <i>k-b-n</i>	<i>ndwa:ka</i>
(11) 'bull'	—	* <i>g-s</i>	<i>gbi:nda-na:key</i>
(12) 'bush'	—	* <i>g-r-</i>	<i>ka:key</i> , <i>sa:mey</i>



(13) 'to buy'	—	* <i>m-s-</i>	<i>doró</i>
(14) 'chicken'	—	* <i>k-z-</i>	m. <i>dye:key</i> , f. <i>dye:któ</i> , pl. <i>dye:kye</i>
(15) 'clean'	—	* <i>ḡ-N</i>	<i>mwa</i> (adj.); <i>fiyó</i> 'to clean'
(16) 'cock'	—	* <i>g-z-</i>	<i>dye:key</i>
(17) 'to come'	<i>shi</i>	* <i>Z-</i>	<i>hwa!</i>
(18) 'cow'	—	* <i>s-</i>	f. <i>naktó</i> , m. <i>na:key</i> (< Fula)
(19) 'crocodile'	<i>giloney</i>	* <i>k-r-m-</i>	—
(20) 'to die'	<i>mbúró</i>	* <i>m-t-</i>	—
(21) 'dog'	—	* <i>k-r-</i>	m. <i>sakey</i> , f. <i>saktó</i> , pl. <i>sakye</i>
(22) 'dove'	—	*( <i>N</i> ) <i>b-l-</i>	<i>gúla</i>
(23) 'to drink'	<i>sóḡó</i>	* <i>s-</i>	<i>sa</i> (Bata)
(24) 'to eat <sub>1</sub> '	<i>zúmó</i>	* <i>z-m-</i>	<i>súḡó</i> (Bata)
(25) 'to eat <sub>2</sub> '	—	* <i>t-</i>	—
(26) 'egg'	—	*( <i>N</i> ) <i>g-(r)</i>	<i>du:ley</i> , pl. <i>ḡo:lye</i>
(27) 'elephant'	<i>'wayey(?)</i>	* <i>g-w-n</i>	—
(28) 'to exceed'	<i>puró</i>	* <i>f-</i>	—
(29) 'eye'	<i>di:tó</i> (f. <i>-tó</i> )	* <i>-d-</i>	—
(30) 'to fall'	—	* <i>t-ḡ-</i>	<i>vúkó</i>
(31) 'fence'	<i>kúzey(?)</i>	* <i>k-r-</i>	<i>ka:nda(?)</i>
(32) 'filth'	<i>riyoktó(?)</i>	* <i>d-kḡ-</i>	<i>ḡiyakuney</i>
(33) 'fire'	—	* <i>w-t-</i>	<i>di:ye</i> (pl.)
(34) 'fish'	<i>'urfey</i> (Bata)	* <i>k-rf-</i>	m. <i>va:key</i> , f. <i>vak-to</i> , pl. <i>va:kye</i>
(35) 'to fly, leap'	—	* <i>p-r-</i>	<i>liyó</i>
(36) 'foot, leg'	—	* <i>s-r-</i>	<i>mbwara</i>
(37) 'to forget'	<i>myentó a ngúró</i>	* <i>m-n-</i>	lit. 'forgetfulness took'
(38) 'four'	<i>fwat'</i> ( <i>fwad'</i> )	* <i>f-ḡ-</i>	—
(39) 'fowl'	<i>dakarey</i>	*( <i> </i> ) <i>k-r-</i>	'bird' (generic)
(40) 'to fry'	—	* <i>Z-r-</i>	<i>hwanó</i>
(41) 'to give'	<i>vó</i>	* <i>b-(r-)</i>	'to hand to'; 'to give (as a gift)' is rendered by the causative <i>vódó</i>
(42) 'to go <sub>1</sub> '	—	* <i>ḡ-</i>	<i>wudó</i>
(43) 'to go <sub>2</sub> away'	—	* <i>d-n-</i>	<i>mudó</i> , <i>munó</i>
(44) 'to go <sub>3</sub> out'	—	* <i>p-t-</i>	<i>dumó</i>

(45)	'goat'	—	*k-	m. <i>hwey</i> , f. <i>ho:tó</i> , pl. <i>hweye</i>
(46)	'hawk'	—	*k-l-r-	<i>nzú-karka-tó</i>
(47)	'head'	—	*k-(n)	<i>ne:</i> , pl. <i>nye:me</i>
(48)	'to hide'	—	*t-k-	<i>ḡó</i>
(49)	'hoof'	<i>kupó-mbwara</i> and <i>kpupó-mbwara</i>	*k-p-	see No. 36
(50)	'horse'	m. <i>duwey</i> , f. <i>duw-</i> <i>tó</i> , pl. <i>dowye</i>	*d-k-	—
(51)	'house'	<i>vúney</i> , pl. <i>vónye</i>	*b-n-	—
(52)	'in-law'	m. <i>sherwey</i> (or <i>shemre</i> ), f. <i>shewtó</i> , pl. <i>shewye</i>	*s-r-	—
(53)	'to kill'	—	*D-k-	<i>ḡ.íló</i>
(54)	'knee <sub>1</sub> '	—	*k-r-m	<i>dúgey</i> , pl. <i>dógye</i>
(55)	'knee <sub>2</sub> '	—	*F-r-m	—
(56)	'knife'	<i>su:ḡgató</i> 'small knife'; <i>su:ḡga</i> 'sword'	*s-G-	—
(57)	'to know'	—	*Z-n-	<i>le:yó</i>
(58)	'to laugh'	—	*m-s-	<i>kyedókó</i>
(59)	'left side'	—	*g-d-	<i>geḡó-lyegdó</i>
(60)	'leopard'	—	*w-r-	m. <i>ja:ra</i> , f. <i>ja:rató</i> , pl. <i>ja:re</i>
(61)	'load'	—	*k-r-	<i>suturey</i>
(62)	'many'	—	*g-d-	<i>furefuri</i> , <i>hananan</i>
(63)	'meat'	—	*s-(w-)	<i>hara</i>
(64)	'to meet'	—	*g-m-	<i>peyó</i>
(65)	'monkey'	<i>buramey</i> '(red- faced) monkey'	*b-d-/b-r-	—
(66)	'moon'	—	*t-r-	<i>likító</i>
(67)	'morning'	—	*d-m	<i>ha:wurey</i> and <i>ha:kpitey</i>
(68)	'mortar (for pound- ing)'	—	*t-(r)m-	<i>su:wey</i>
(69)	'mud (for building)'	—	*t-ḡ-	<i>sḡḡwey</i>
(70)	'name'	—	*s-m-	<i>kwakey</i> , pl. <i>kwa-</i> <i>kye</i> (also means 'ear')
(71)	'neck'	<i>wura</i> , pl. <i>wure</i>	*w-r-	—
(72)	'night'	—	*b-d-	<i>túkótó</i>
(73)	'nose'	<i>cinney</i> , pl. <i>ce:nye</i>	*t-n	—

(74) 'oil'	<i>ma:rey</i>	* <i>m-r</i>	—
(75) 'ostrich'	—	* <i>t-l-m</i>	<i>ɓakaró-vúra</i> (lit. 'bird of world')
(76) 'a place'	—	* <i>w-r-</i>	<i>ha:</i>
(77) 'to plant'	—	* <i>s-k-</i>	<i>ɲgubó, laga</i>
(78) 'pot <sub>1</sub> '	—	* <i>β-(r-)</i>	<i>ɗwató</i>
(79) 'to pound grain'	<i>súwa</i>	* <i>s-(r)p-</i>	—
(80) 'to pour'	—	* <i>z-( )</i>	<i>púkó</i>
(81) 'to pull'	—	* <i>d-</i>	<i>ɲgóló gwe:só</i> 'to pull up'
(82) 'to put'	—	* <i>s-(k-)</i>	<i>dúkó</i> , pl. <i>púkó</i>
(83) 'quarrel'	—	* <i>k-r-</i>	<i>nyató</i>
(84) 'ram'	—	* <i>(N)g-m</i>	m. <i>mbaga</i> , f. <i>mba-gató</i> , pl. <i>mbage</i>
(85) 'red'	—	* <i>d-z-</i>	<i>kpalam, miso-miso, biɲ-biɲ</i>
(86) 'road, path'	<i>tufey</i>	* <i>t-(r)b-</i>	—
(87) 'root'	—	* <i>s-rw-</i>	<i>kwi:rey</i> , pl. <i>kwe:-rye</i>
(88) 'to say <sub>1</sub> '	—	* <i>p-r-</i>	<i>ɓóyó</i> 'to relate'
(89) 'to say <sub>2</sub> '	<i>sa</i> (Bata <i>ca</i> ) (also <i>sa ati</i> or <i>sa atú</i> )	* <i>t-( )-</i>	—
(90) 'to see <sub>1</sub> '	<i>na</i>	* <i>n-</i>	—
(91) 'to see <sub>2</sub> '	—	* <i>l-</i>	—
(92) 'sheep'	—	* <i>t-m-k-</i>	m. <i>mbaga</i> , f. <i>mba-gató</i> , pl. <i>mbage</i>
(93) 'six'	—	* <i>m-k-</i>	<i>tukolta:ka'</i>
(94) 'skin <sub>1</sub> '	—	* <i>(k-)s-m</i>	<i>sula:rey</i> , pl. <i>sola:-rye</i>
(95) 'skin <sub>2</sub> '	—	* <i>p-n-</i>	—
(96) 'slave'	—	* <i>d-b-</i>	<i>kúsa</i> , pl. <i>kúse</i>
(97) 'sleep'	<i>shintó</i> , pl. <i>shéntó</i>	* <i>(w-)s-n-</i>	—
(98) 'sore'	—	* <i>t-( )d-</i>	<i>ɓókey</i>
(99) 'spear'	—	* <i>(N)g-s-</i>	<i>kúfey</i>
(100) 'to spit'	<i>tofó</i>	* <i>t-f-</i>	<i>shedó, poyed dó</i>
(101) 'to steal'	—	* <i>m-r-</i>	<i>huró</i>
(102) 'stick'	—	* <i>z-l-</i>	<i>kada</i> , pl. <i>kade</i>
(103) 'stone <sub>1</sub> '	m. <i>fara</i> , f. <i>farató</i> , pl. <i>fare</i>	* <i>p-r-</i>	—
(104) 'stone <sub>2</sub> '	—	* <i>(N)d-G-</i>	—
(105) 'sun; day'	<i>fórey</i>	* <i>F-t-</i>	—
(106) 'ten'	—	* <i>g-m-</i>	<i>bów'</i>

(107)	'termite'	—	*d-( )δ-	<i>gbakaluruwe</i> (‘soldier ants’)
(108)	'three'	<i>mwa:kún'</i>	*k-n-	—
(109)	'to tire'	—	*(N)g-m-	<i>δwa:</i>
(110)	'tomorrow'	—	*d-(r-)	<i>kúda</i>
(111)	'tooth'	<i>lintó</i> , pl. <i>lye:nye</i>	*s-n	—
(112)	'to turn up-side down'	<i>kupó</i>	*k-(r)p-	<i>ndula</i>
(113)	'two'	—	*s-r-	<i>kpe'</i>
(114)	'to warm'	—	*d-m-	<i>shifa</i> , <i>cífa</i>
(115)	'to wash, bathe'	—	*b-n-	<i>yobó</i> , <i>subwa</i>
(116)	'water'	—	*( )-m	<i>habye</i> , <i>mbe</i>
(117)	'what?'	<i>munó</i> ‘what is it?’	*m-(n-)	—
(118)	'who?'	<i>wonó</i> ‘who is it?’	*w-(n-)	—
(119)	'wing'	—	*(k-)p-k	<i>ba:pa</i>

## LIST II

(120)	'arrow'	—	*k-b-	<i>ngolmbotó</i>
(121)	'to beat <sub>1</sub> '	—	*w-d-	<i>lyebó</i> , <i>biyó</i> , <i>wiyó</i>
(122)	'to beat <sub>2</sub> '	—	*d-G-	<i>δina</i> , <i>fula</i> , <i>dweló</i> , <i>fuda</i>
(123)	'belly'	—	*t-mb-	<i>je:dye</i>
(124)	'bird'	—	*k-d-(n)	<i>δakarey</i>
(125)	'bitter'	—	*d-D	<i>kwat</i>
(126)	'body'	<i>sutó</i>	*z-	—
(127)	'bone'	—	*W-s-	<i>úley</i>
(128)	'bow'	<i>ragey</i>	*r-gh-	—
(129)	'to do'	—	*k-	<i>da</i>
(130)	'fat'	—	*k-d-r	<i>kuli-kuli</i>
(131)	'good'	—	*g-N-	<i>hula</i> , <i>hula-hula</i>
(132)	'guinea fowl'	—	*z-b-(n)	<i>kwadantó</i>
(133)	'hair'	—	*g-s-	<i>shewtó</i> , pl. <i>shewe</i>
(134)	'to hoe'	—	*h-d-	<i>hawo</i>
(135)	'iron'	—	*y-m	<i>ta:</i> (‘metal, mo- ney’)
(136)	'one'	<i>hido'</i>	*G-D-	—
(137)	'pot <sub>2</sub> '	—	*D-gh-l-	m. <i>dwa</i> , f. <i>dwató</i> , pl. <i>dwe</i>
(138)	'pot <sub>3</sub> '	—	*t-k-N	—
(139)	'rat'	m. <i>hi:mey</i> , f. <i>him- tó</i> , pl. <i>hye:mye</i>	*G-s-m	—
(140)	'to rise'	—	*t-s-/s-t-	<i>madó</i>

(141) 'to tear'	—	* <i>ḡ</i> -( <i>r</i> ) <i>k</i> -	<i>suró</i>
(142) 'to tie'	—	* <i>g</i> - <i>nE</i>	<i>ḡóyó</i>
(143) 'to wean, take away'	—	* <i>d</i> -( ) <i>k</i> -	<i>ira</i>
(144) 'white'	<i>pwat-pwat</i>	* <i>p-r-t</i> -	<i>kuy</i>

An examination of the items in Column 2 shows the following correspondences between the Proto-Chadic consonants and the Bachama ones:

P-C initial <i>p</i>	8. <i>fúló</i> ; 103. <i>fara</i> ; 144. <i>pwat-pwat</i> .
non-in. <i>p</i>	49. <i>kupo</i> ; 79. <i>suwo</i> ; 112. <i>kupó</i> .
initial <i>b</i>	41. <i>vó</i> ; 51. <i>vúney</i> ; 65. <i>buramey</i> .
non-in. <i>b</i>	86. <i>tufey</i> .
initial <i>ḡ</i>	7. <i>ḡiyó</i> .
non-in. <i>ḡ</i>	none.
initial <i>t</i>	73. <i>ciney</i> ; 86. <i>tufey</i> ; 89. <i>sa/ca</i> ; 100. <i>tofó</i> .
non-in. <i>t</i>	20. <i>mbúró</i> ; 105. <i>forey</i> ; 144. <i>pwat-pwat</i> .
initial <i>d</i>	4. <i>zambe</i> ; 32. <i>riyoktó</i> ; 50. <i>duwey</i> .
non-in. <i>d</i>	29. <i>di:tó</i> ; 65. <i>buramey</i> (also <i>b-r</i> -).
non-in. <i>D</i>	136. <i>hidó</i> '.
initial <i>d</i>	none.
non-in. <i>d</i>	38. <i>fwat</i> ' ( <i>fwad</i> '?).
initial <i>k</i>	19. <i>giloney</i> ; 31. <i>kúzey</i> ; 34. (Bata <i>'urfey</i> ).
non-in. <i>k</i>	39. <i>bakarey</i> ; 32. <i>riyoktó</i> ; 50. <i>duwey</i> .
initial <i>g</i>	27. <i>'waney</i> (initial syllable lost).
non-in. <i>g</i>	none.
non-in. <i>gh</i>	128. <i>ragey</i> (P-C voiced velar fricative).
initial <i>G</i>	136. <i>hidó</i> '; 139. <i>hi:mey</i> .
non-in. <i>G</i>	56. <i>su:ngató</i> .
initial <i>f</i>	28. <i>puró</i> ; 38. <i>fwat</i> '.
non-in. <i>f</i>	34. <i>'urfey</i> ; 100. <i>tufey</i> .
initial <i>F</i>	6. <i>fudó</i> ; 105. <i>fórey</i> .
non-in. <i>F</i>	none.
initial <i>s</i>	23. <i>sóbó/sa</i> ; 52. <i>sherwey</i> ; 56. <i>su:ngató</i> ; 79. <i>suwó</i> ; 111. <i>lintó</i> .
non-in. <i>s</i>	8. <i>fúló</i> ; 97. <i>shintó</i> ; 139. <i>hi:mey</i> .
initial <i>z</i>	24. <i>zúmó</i> ; 126. <i>sutó</i> .
non-in. <i>z</i>	none.
initial <i>Z</i>	17. <i>shi</i> .
non-in. <i>Z</i>	none.
initial <i>m</i>	20. <i>mbúró</i> ; 37. <i>myentó</i> ; 74. <i>ma:rey</i> ; 117. <i>múnó</i> .
non-in. <i>m</i>	19. <i>giloney</i> ; 24. <i>zúmó</i> ; 139. <i>hi:mey</i> .
initial <i>n</i>	90. <i>na</i> .
non-in. <i>n</i>	37. <i>myentó</i> ; 51. <i>vúney</i> ; 73. <i>ciney</i> ; 97. <i>shintó</i> .

initial <i>N</i>	none.
non-in. <i>N</i>	4. <i>zambe</i> .
initial <i>w</i>	71. <i>wura</i> ; 97. <i>shintó</i> (lost syllable); 118. <i>wóno</i> ?
non-in. <i>w</i>	27. <i>'wayey</i> .
initial <i>r</i>	128. <i>ragey</i> .
non-in. <i>r</i>	19. <i>giloley</i> ; 31. <i>kúzey</i> ; 34. <i>'urfey</i> ; 39. <i>bakarey</i> ; 41. <i>vó</i> ; 52. <i>sherwey</i> ; 65. <i>buramey</i> ; 71. <i>wura</i> ; 74. <i>ma:rey</i> ; 79. <i>suwó</i> ; 86. <i>tufey</i> ; 103. <i>fara</i> ; 112. <i>kupó</i> ; 144. <i>pwat-pwat</i> .
initial <i>l</i>	none.
non-in. <i>l</i>	none.

It appears from the correspondences shown in these few examples—and they have been selected purely on the basis of Newman and Ma's lists—that there is reason for including Bachama in the Biu-Mandara group (or in the Plateau-Sahel group, as the difference between the groups has not been taken into account), but the addition of my examples could bring modifications into the set of Proto-Chadic consonants which they have reconstructed. In further work, I should be more interested perhaps to establish phonological correspondences between Bachama and one of the other languages, say Tera, rather than looking primarily at the problem of working towards a proto-language. But the first task is to produce a detailed phonological study of Bachama itself.

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\* Due to lack of time this paper was not discussed.

**VII**

**GENERAL PAPERS**





## WHAT'S IN A NAME?

A. N. TUCKER

This paper is actually a restatement, for the benefit of the present conference, of the stand recently taken by Miss Bryan and myself (Tucker 1967b; Tucker and Bryan 1966) in the matter of nomenclature for the language family under discussion here, known in most circles as the 'Semitic-Hamitic' or 'Hamito-Semitic' family. These terms were obviously inspired by the study of Genesis 10, and have been used freely in both a racial and linguistic sense, so some reference here to their Biblical origin would not be out of place. According to this, the descendants of the three sons of Noah spread over the Fertile Crescent as follows:

"The sons of Shem: Elam, Asshur, Arpachshad, Lud, and Aram" (10:22); "the territory in which they lived extended from Mesha in the direction of Sephar to the hill country of the east" (10:30).

"The sons of Ham: Cush, Egypt, Put, and Canaan" (10:6); "Cush became the father of Nimrod" (10:8); "the beginning of his kingdom was Babel, Erech, and Accad, all of them in the land of Shinar" (10:10); "from that land went forth Asshur, and built Nineveh, Rehoboth-Ir, Calah, and Resen" (10:11); "Egypt became the father of Ludim, Anamin, Lehabim, Naphtuhim" (10:13); "Pathrusim, Casluhim (whence came the Philistines), and Caphtorim" (14); "Canaan became the father of Sidon his first-born, and Heth" (10:15); and the Jebusites, the Amorites, the Girgashites (16), the Hivites, the Arkites, the Sinites (17), the Arvadites, the Zemarites, and the Hamathites (18); "and the territory of the Canaanites extended from Sidon, in the direction of Gerar, as far as Gaza, and in the direction of Sodom, Gomorrah, Admah, and Zeboiim, as far as Lasha" (10:19).

"The sons of Japheth: Gomer, Magog, Madai, Javan, Tubal, Meshech, and Tiras" (10:2); "from these the coastland peoples spread" (10:5).

It would thus appear that the descendants of Shem were congregated mostly in the Mesopotamian area, those of Ham in Palestine and North Africa, and those of Japheth in the Caucasus area.

The term 'Semitic' it appears was coined by Schlözer in 1781, to describe the languages of the descendants of Noah's eldest son. As a conventional linguistic term it is now everywhere accepted—though an exception has to be made in the case of

Elam (regarded as being related to Sumerian) and Lud (about which nothing linguistic is apparently known). As an anthropological or sociological term it has recently been somewhat bedevilled by politics, in that some writers have been known to refer to the Arabs as being 'anti-Semitic'! But the linguistic boundaries of the Semitic languages have always been sharply defined, even though the languages themselves are spoken all over the Fertile Crescent and have even penetrated Africa.

It was inevitable, bearing in mind the popular attitude at the time, that Noah's second son should also be regarded as the founder of a race and language family. At a relatively early stage Ham and certain of his descendants came to be regarded as black men, confined in residence to Africa—this despite the fact that Canaan was also a son of Ham, and the Canaanites lived in Asia Minor and spoke a Semitic language. Certainly by Lepsius' time the term 'Hamitic' was taken to cover Berber, Ancient Egyptian and the non-Semitic languages of the Horn of Africa, which themselves came to be called 'Cushitic' (Lepsius 1880). The choice of Cush as the father of a language group was not a very happy one when it is remembered that Accadian is also a Semitic language of Asia Minor, in spite of the relationship of Accad to Nimrod, son of Cush!

Some adjustment was evidently called for just at a period in our history when the Bible was coming in for increased criticism as the ultimate historical authority. But the overall unity of these languages was recognized at an early date—hence the terms 'Semito-Hamitic' or 'Hamito-Semitic'; hence also the terms 'Chado-Hamitic' and 'Nilo-Hamitic' for suspected hybrids. This overall unity did not apparently extend to Noah's third son and his descendants. They, having emigrated towards the Caucasus area, were presumably held responsible for the rest of the world's languages.

In recent years the feeling has grown in linguistic circles, a feeling shared by Marcel Cohen himself (1947), that the linguistic criteria for dividing the Semitic and Hamitic languages into two distinct blocks do not exist. All these languages have certain common elements in vocabulary, morphemes and patterns of grammatical behaviour (especially in verb conjugation), which are characteristic of the whole family. But if one discounts those elements in the 'Hamitic' languages which are also characteristic of the 'Semitic' languages, there is nothing left that could be called typically 'Hamitic', that could, for instance, link Berber, Ancient Egyptian and Cushitic into a unity standing over against 'Semitic'. All the evidence points to at least five units of equal hierarchical standing, though from the historico-linguistic point of view, some authorities like to regard the Semitic languages as being 'younger' than the rest—even though bearing the name of the eldest son of Noah.

This situation was well realized by Greenberg (1955; 1963) when he coined the term 'Afroasiatic' with a linguistic-geographical connotation. G. P. Murdoch (1959), on the other hand, apparently unwilling to let a good name go a-begging,

suggested 'Hamitic' as the family name—thus implicitly promoting Ham to generic superiority over his elder brother Shem and to the exclusion of Father Noah!

Greenberg's sub-division is into: A. Semitic, B. Egyptian, C. Berber, D. Cushitic, E. Chad. Under the last-named section he is still challenged by some scholars who feel that he has over-simplified the situation and that in reality two units are concerned—Chadic languages proper, and Chado-Hamitic languages, i.e. Chadic languages under 'Hamitic' influence. Much field work remains to be done in this area. Our own investigations in East Africa suggest the possibility of yet other sections (Tucker 1967a).

Concerning the name 'Afroasiatic', our feeling is that such a small corner of Asia is involved that its inclusion by Greenberg in the family title is hardly warranted; further it might be taken as supporting Mlle Homburger's theory (not shared by Greenberg) of the genetic relationship of the Dravidian languages of India to these and other languages of Africa.

Consequently we have resuscitated the term 'Erythraic' (Tucker and Bryan 1966, 1), with due acknowledgement to Reinisch,<sup>1</sup> as appropriate to this family of languages, situated as it is on both sides of the Red Sea, the latter thus playing the role of a hinge. Or, if one wishes to borrow an analogy from entomology, one could consider the Erythraic family as an enormous lop-sided butterfly pinned to a board, its body constituting the Red Sea. This term is not without its disadvantages, however, the main one being the possibility of confusion with the country Eritrea and its adjective Eritrean. Here again political implications could impinge.

Whatever shortcomings the term 'Hamitic' might have for philologists, the term 'Hamite' has been a major stand-by for students of race. It had long been known that Africa contained ethnic types completely at variance with the Negro type. The most outstanding of these types was to be found in the speakers of the so-called 'Hamitic' languages, who were regarded as early invaders from Asia Minor. R. A. Oliver and J. D. Fage (1962) speak of "a race akin to the Caucasians—it is called by Leakey 'proto-Hamitic'—and almost certainly it had evolved its special characteristics in south-west Asia, from where some of its members moved into Africa. These Caucasoid proto-Hamites, who buried at least some of their dead and thus left evidence of their physical features, practised an extremely significant material culture called Capsian, after the type-site Gafsa in Tunisia".

<sup>1</sup> (1873), xv f.: "Ich gebrauche den Namen 'südafrikanische Sprachen' für die gruppe welche man bisher 'Bantusprachen' genannt hat. ... Ich gebrauche ferner den Namen 'erythraische sprachen' für die gruppe welche man bisher in die 'semitischen' und 'hamitischen' unterschieden hat. Ich habe bereits im Eingange kurz angedeutet dass durchaus kein einziger Grund vorliegt diese auf das engste zusammen gehörigen Sprachfamilien auseinander zu reissen. Ein weiterer Beweggrund jenen Namen zu wählen, lag für mich in dem Umstande dass ich die Länder zu beiden Seiten des erythraischen Meeres in Folge von Gründen die ich im geschichtlichen Teile auseinander setzen werde, für die eigentliche Heimat der sogenannten semitisch-hamitischen Völkerfamilie ansehe" (I-V-1873).

The main characteristics of these 'Hamitic' invaders were that they were tall, dolichocephalic, straight-nosed, thin-lipped, 'aristocratic'-looking, and kept cattle. It soon became apparent, however, that these characteristics were also to be found among speakers of other languages. Though Meinhof (1912) staked his considerable linguistic reputation in an attempt to prove that Fula, Hottentot and Maasai were genetically related to the more orthodox 'Hamitic' languages Hausa, Schilh, Bedaue and Somali, his book's supplement on *Hamitische Typen* by Felis von Luschan extended the net to cover Egyptian Pharaohs on the one hand and Bahima herdsmen, coastal Swahili and even Zulus on the other.

C. G. Seligman (1967, 61) seems to have built on this foundation. According to him:

the Hamites—who are 'Europeans', i.e. belong to the same great branch of mankind as the whites<sup>2</sup>—are commonly divided into two great branches, Eastern and Northern.

(1) The Eastern Hamites comprise the ancient and modern Egyptians ..., the Beja, the Berberines (Barabra or Nubians<sup>3</sup>), the Galla, the Somali, the Danakil, and (though mixed with Semites and Negroes) most Ethiopians.

(2) The Northern Hamites include the Berbers of Cyrenaica, Tripolitania, Tunisia, and Algeria (often conventionally distinguished as Libyans), the Berbers of Morocco, the Twareg and Tibu<sup>3</sup> of the Sahara, the Fulbe<sup>3</sup> of the Western Sudan, and the extinct Guanche of the Canary Islands.

But he also speaks (1967, 101) of 'Hamiticized Negro' under which he lists (1) the Nilo-Hamites, (2) the Nilotes and (3) certain Bantu—the result of incoming waves of Hamitic blood mixing with Negro stock:

At first the Hamites, or at least their aristocracy would endeavour to marry Hamitic women, but it cannot have been long before a series of peoples combining Negro and Hamitic blood arose; these, superior to the pure Negro, would be regarded with disdain by the next incoming wave of Hamites and be pushed farther inland to play the part of an incoming aristocracy *vis-à-vis* the Negroes on whom they impinged. And this process was repeated with minor modifications over a long period of time, the pastoralists always asserting their superiority over the agriculturists, who constantly tended to leave their own mode of life in favour of pastoralism or at least to combine it with the latter. The end result of one series of such combinations is to be seen in the Zulu, of another in the Ganda, while an even more striking result is offered by the symbiosis, to use a biological term, of the Huma of Ankole and the Iru. The Huma, a tall, cattle-owning aristocracy, with narrow noses and faces, so unlike Negro (though they always have Negro hair) that Johnston when he first saw them thought they were Egyptian soldiers left behind by Emin Pasha, live in the country of the shorter, broader-faced Negro Iru ...

A similar situation is to be found between the Batuutsi and the Bahutu of Ruanda, and the Lui and Dupi among the Bari who, however, are already 'Nilo-Hamitic'—or 'Half-Hamite'—according to Seligman elsewhere (1932, 14).

<sup>2</sup> In the original edition the author had written that the Hamites "are Caucasians, belonging to the same great branch of mankind as almost all Europeans". Surely an usurpation of Brother Japheth's demesne.

<sup>3</sup> Speaking, however, non-'Hamitic' languages.

The linguistic value of the term 'Nilo-Hamitic' has been exploded by Greenberg, and the term itself has been given up by Tucker and Bryan<sup>4</sup> in favour of 'Paranilotic'—but anthropologists have yet to be convinced that it has no racial significance.

As for the term 'Hamitic'—it has long satisfied the needs of historians, anthropologists, archaeologists, journalists and popular travel-book writers. Can anyone think of a suitable alternative?<sup>5</sup>

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<sup>4</sup> (1966, 1), after an abortive attempt eleven years previously to compile a list of 'Hamitic' criteria in these languages; see Tucker and Bryan (1956), 154-156.

<sup>5</sup> Seligman (1967, 63) mentions the term 'Erythriotes' in use in some circles to distinguish the 'Eastern Hamites' from the 'Northern Hamites' ('Libyans').

Westermann, D., and M. A. Bryan,

1952 *The Languages of West Africa, Handbook of African Languages* (London).

#### DISCUSSION

ADAMS: Might I suggest that, rather than Afroasiatic, one might choose the name Afro-Arabian. This group occupies only a small part of Asia and we tend to think of other language groups under the general heading of Asiatic, whereas it is principally Arabia that is the part of Asia in which languages of this group are spoken.

TUCKER: What I was really wanting to find out was the reaction of this conference to the suggestion, put forward by Reinisch in the first place and then later on by us, of the term Erythraic. Also, does the conference consider the term Hamitic to have lost its value entirely, or has it still some value left? This is particularly the case for the Chadic languages where the German School I understand either do or did divide the languages into two, Chadic and Chado-Hamitic. The very brief examination I have been able to devote to the information, and especially that provided by Dr. Jungraithmayr at this conference, leads me to believe that they all, at least where the verb conjugation is concerned, seem to behave in the same way whether you call them Chadic or Chado-Hamitic. So that if there is no need for a composite name, that would appear to take away the last reason for the use of the term Hamitic. Because otherwise it seems rather ridiculous to say Semito-Hamitic when there is no Hamitic.

ANDRZEJEWSKI: I would be very much in favour of the term Erythraic instead of Hamito-Semitic, because the grouping of the languages does not justify such a dichotomy. It is also unfortunate that by implication there is a suggestion that the speakers belong to two distinct racial groups, which is challenged by some physical anthropologists.

WOLFF: I would like to reply to what Professor Tucker has just said regarding the Chadic languages, since Dr. Jungraithmayr is unfortunately absent today. As Professor Tucker said there still remains the distinction within the Chadic branch between Chado-Hamitic and Chadic, first made by Professor Lukas in 1936 and repeated in the *Handbook* (Westermann and Bryan 1952). It was Greenberg who, between 1950 and 1966, grouped all the languages together as Chadic. Newman and Ma adopted this entity of Chadic but split it up again into the Plateau-Sahel group and the Biu-Mandara group, where the Biu-Mandara group reflects what Lukas had called Chadic and the Plateau-Sahel group what he had called Chado-Hamitic. It is important I think to note that Lukas used more or less typological criteria (the existence of gender, for example, the presence of lateral fricative sounds,

the absence of velar labials), but we now hear from Mr. Carnochan that these do not hold for Bachama, which is a member of the Chadic proper or Biu-Mandara group. Hoffmann also mentions in his *Margi Grammar* that he is not at all sure whether this language belongs to the Chadic group. Again Mr. Parsons in his paper says "I definitely feel that my place is not among the Chadicists", and two pages later "Hausa cannot any longer justifiably be classed as a Chadic language". Dr. Skinner in his paper on the problem of gender in Hausa said that "Hausa is in some ways closer to Berber than to other Chadic languages". Regarding Dr. Jungraithmayr's paper, his dual system of aspects does not hold for the members of the Biu-Mandara group where there is nothing equivalent. So I do think it is quite clear that Hausa can no longer serve as the representative of the whole language family.

KNAPPERT: Professor Tucker and Miss Bryan have stated (Tucker and Bryan 1956; 1966) that they found no justification for the term Nilo-Hamitic on the grounds that it contained no 'Hamitic'; they therefore preferred to call it para-Nilotic. I myself when working on Nilotic found only eleven words that were clearly similar in the Nilotic and Nilo-Hamitic language groups, so that it would appear that in Nilo-Hamitic there is no Nilotic either! Greenberg has lumped the two together—he calls them all 'Nilotic' and speaks only of 'South-Western' and 'Eastern Nilotic'—but his evidence is certainly not convincing to me.

SKINNER: The question of names is always a highly emotive one, but I have two brief suggestions. Either from the etymological point of view one might adopt the common elements and refer to the group as the Mitic group, or one might adopt the genealogical approach and refer to it as the Noahitic group.

CARNOCHAN: In some of the societies in which the languages we are examining are spoken, when a child is born a large number of relatives and people visit the child and give it a number of names: 'We are thankful to God', 'God has answered our prayer', 'What a fine market it was', and so on. And in due course as the child grows up some of these names fall away and one perhaps sticks. I think that we should let this same sort of process take place in our own studies. We should not be too anxious to adopt a mono-approach and refer to these languages all in exactly the same way. As more work is done on the relationship between the individual languages a large number of smaller groupings will emerge and possibly these will suggest later on the best way of dealing with the problem.





SOME ASPECTS OF THE PRESENT STATE  
OF HAMITO-SEMITIC STUDIES

B. S. J. ISSERLIN

As in the case of Indo-European so also in Hamito-Semitic research, the field of enquiry can either be regarded as being restricted to purely linguistic questions or it may be assumed that it can be extended with profit into such subsidiary or allied disciplines as onomastic research (involving both the names of persons and places), comparative religion, archaeology, and both social and physical anthropology. The most recent comprehensive study on the Semitic-Hamitic field, namely Diakonoff's *Semito-Khamitskie Yazyki* (Moscow 1965), deals exclusively with the linguistic side; no comprehensive study taking in also the other fields mentioned has appeared since Barton's *Semitic and Hamitic Origins* (Philadelphia 1934). A comparison between the two books will bring out fairly clearly where most progress has recently been made, and how our overall as well as our more detailed ideas have been modified; at the same time such a comparison may point to a number of questions which may be asked but which have not yet been answered.

In the linguistic sphere the work done during the last generation has led, first of all, to a very significant new classification. Cohen and Meillet (1952) and Greenberg (1955) have, together with others, been instrumental in pioneering a now widely accepted view according to which the Hamito-Semitic languages should be grouped into five separate although interrelated families of equal standing: Semitic, Ancient Egyptian, Cushitic, Berber, and Chad. This new framework has been strengthened by the comparative study of common elements, both structural (root formation, noun, verb, pronoun) and lexical, existing between the five main groups mentioned,<sup>1</sup> and this in turn has led to the suggestion that sub-groupings of historical significance can be shown to exist. Diakonoff (1965, 99) in particular proposed to treat Cushitic and Chad as members of a more archaic southern group while Ancient Egyptian, Berber, and Semitic (in which, for example, the trilateral root is far more common) all formed part of a more progressive northern entity. In detail, here as elsewhere, many questions remain. Thus, for example, the hypothesis that the Old Libyan verbal system can be assumed to have been almost identical with the old Semitic

<sup>1</sup> Cf., besides Greenberg (1955), M. Cohen (1947)—to refer only to two outstanding examples.

verbal system found in Akkadian as proposed by Rössler (1951, 101-107, 366-373; 1952, 121-150; 1950, 461-514), has not found universal assent. Yet in general the more limited or general isoglosses which have led to suggestions such as those made by Diakonoff are impressive, and they have helped to create a much clearer picture of the essential similarities and differences within Hamito-Semitic. Where the question of development is concerned, it seems to this writer that the concept of the Hamito-Semitic area in general as a zone across which linguistic innovations spread from various centers to varying extents and at varying rates, might be found useful; it would for instance be possible to explain a number of striking lexical isoglosses linking the spatially widely separated Chad and Cushitic regions (Diakonoff 1965, 50) as being the result of linguistic diffusion across a (now broken) continuum. This concept has in fact been introduced and developed with profit in the more restricted field of Semitic linguistics (the most intensively worked area within the Hamito-Semitic field) which offers a number of valuable points for our general survey.

Semitic language studies during the last forty years have benefited from two special factors. Firstly, far more exact detail is now available as the result of careful analytical studies about languages known earlier, and this has made clearer both the existence of regional variations and of historical developments within such language boundaries; secondly, languages which earlier were entirely unknown (Ugaritic) or little known (Amorite) are now reasonably well documented and analysed.<sup>2</sup> This has made possible diachronic and synchronic studies of an accuracy unmatched in the Hamitic sphere. The detailed description of the rate of linguistic changes in space and time, as in the case of Canaanite dialects (Harris 1939) or of North West Semitic in general (Garbini 1961), may be referred to as examples. This has in turn given rise to new views on the history of the Semitic language group as a whole. In a paper of unusual interest, Rabin (1963, 104-115) has advocated the concept of a fairly stable Semitic linguistic territory within which innovations spread unequally from various centers. Building upon such foundations, Garbini (1965, 9) has further postulated a linguistically active zone, including the North Syrian desert fringe and Arabia, where innovations like the West Semitic tense system developed and from which they spread. Arabic would on the basis of this view be one of the more recent Semitic languages (as compared, for example, with Akkadian) and not the most faithful surviving representative of the proto-Semitic from which the Semitic languages developed and spread out in a number of waves as postulated by earlier theories. Such views implicitly re-open the question as to the original home of 'proto-Semitic' (now often seen as a grouping of inter-connected dialects rather than a unified language). While the view that Arabia was the main home of the Semites in historical times has been repeatedly and strongly defended by Moscati (1959) in recent years (though with very important limitations where early periods about

<sup>2</sup> For convenient summaries of the present position see, for example, Moscati *et alii* (1964); Levi della Vida (1961).

which evidence is lacking are concerned), alternative suggestions envisaging Syria-Palestine<sup>3</sup> or Northern or Eastern Africa (Diakonoff 1965, 101-102; Murtonen 1967, 74) have also been revived, when the question has not been dismissed altogether as meaningless (Garbini 1965, 7-8). Here we should also mention the suggestion made by Garbini (1965, 13-14) that the Hamitic languages are the product of a superposition of intrusive Semitic and Asiatic elements upon various pre-existing African languages, a view in direct and far reaching opposition to theories implying an African derivation for Semitic. Concerning the time scale involved in any of these theories, we are reduced to guess-work (although sometimes very shrewd). No glotto-chronological analysis to determine the rate of development of Hamito-Semitic, or of the divergence of its branches, has so far been published.<sup>4</sup>

The progress made in the field of Semitic studies has thus in some ways not yet been instrumental in clarifying the early history of Hamito-Semitic relations; but on the other hand, through the distinction between older and younger Semitic languages, it has made the drawing of a unified picture for the whole of the Afro-asiatic language family easier and has provided useful ideas and models for research in that sphere. It remains to be said that, where the question of the relations between Hamito-Semitic and other language families is concerned, we are still in the stage of preliminary and preparative work although, for example, the recent listing of possible traces of a 'class' system within Hamito-Semitic noun formation (Diakonoff 1965, 52) is suggestive.

We must now turn to the question of whether evidence of a non-linguistic kind can contribute towards solutions not obtainable from purely linguistic research, through the establishment of parallelisms of a suggestive nature between linguistic and non-linguistic evidence.

In reviewing the various kinds of evidence which might be considered here, we can be brief under a number of headings. Thus onomastic research is fairly developed on the Semitic side where names of persons are concerned and a beginning has been made with overall place-name research,<sup>5</sup> but the Hamitic side has not been investigated to a similar extent and no recent study of Hamito-Semitic onomastics exists. Similarly, Semitic religion has been studied intensively and some interesting conclusions as to presumably common early Semitic deities have recently been drawn (Moscatti 1958, 119ff.), but there appears to be no corresponding work for the Hamitic side and no recent overall study of Hamito-Semitic beliefs. Additional comprehensive study in the field of social anthropology likewise remains to be done. In the field of physical anthropology a review of published evidence about early skeletal material seems to lead to no definite conclusions which would show that the Hamito-Semitic

<sup>3</sup> Fronzaroli (1960, 123-144) thinks of N. Syria; Garbini (1965, 1-15) seems inclined to consider Syria-Palestine, and in this he is to some extent paralleled by Henning (1968, 151).

<sup>4</sup> Though the present Colloquium includes two papers employing this method.

<sup>5</sup> The literature dealing with personal names is very extensive; for a summary see Isserlin (1965). For Semitic place-names the only general study to date is Isserlin (1956), 83-110.

speaking area corresponds essentially to the zone of distribution of any one physical type, for Mediterranean and Euroform types both occur from early times in the region as do variations of skull shape even within comparatively limited ranges of space and time.<sup>6</sup> Negroid features have been noticed in some early local contexts, but their significance is not clear (Childe 1952, 42, 46). Similarly the study of the distribution of blood groups (Mourant and Kopec 1958, 268-270) does not help to isolate the Hamito-Semitic region from its neighbours, and theories of complicated migrations involving areas both inside and outside the Hamito-Semitic speaking zone may be needed to explain the present distribution map (Briggs 1957, 195ff.).

On the other hand a consideration of the results of recent archaeological work will offer interesting suggestions for our survey, although there are difficulties which remain to be resolved. Here we must refer in particular to the discovery of a number of sites scattered throughout Arabia from Qatar to the south west fringes of the Rub' al Khali desert (Kapel 1967), where the elements of a stone age culture, prominently including among its utensils bifacially worked and pressure flaked arrow-heads (lozenge shaped, or tanged, or tanged and winged), have been found. The late Professor Zeuner (1954) noted that this culture was also represented by as yet unpublished finds in Mesopotamia, but that essentially it was related to the 'Neolithic' cultures of the Sahara, the Fayum, and East Africa and that its ancestry seemed to be African. The total area occupied by this Afro-Asian cultural complex thus corresponds reasonably well to the Hamito-Semitic speaking region, though it excludes Palestine where the Mesolithic Natufian and later the Neolithic Tahunian flint cultures were at home. The former has, however, been shown to have some relations with North Africa<sup>7</sup> and the latter actually extended into the Qatar region of Arabia (Kapel 1967, 16). If a cultural equivalent to the Hamito-Semitic speaking populations is to be looked for, this Arabian-African culture suggests itself. (This is not to say that this cultural complex may not also have included the speakers of other languages, like ancestral Tedu (Briggs 1957, 197-198). We must also remember that the whole archaeological complex includes agriculturalists as well as pastoralists, hunters, and fishermen. Later distinctions, such as that between Bedu and Ḥaḍar in Arabia, do not yet apply and must have crystallized slowly among the Semites. Where Semitic origins in particular are concerned an African source seems likely but East Africa is probably to be excluded since the local cultural facies does not typically include the forms of arrow heads prevalent in Arabia (Clark 1954, 330).

It is, however, just when we try to cross the boundary from Hamito-Semitic to Semitic proper that we are faced with a difficulty. This will best be understood if we begin by considering the archaeological situation in Syria-Palestine at a time when Semitic speakers (Amorites, Canaanites) are known to have lived in those

<sup>6</sup> See Ferembach (1959), 65ff., 221ff.; on physical types represented at various early Egyptian sites cf. further Childe (1952), 40.

<sup>7</sup> For Tasian and Natufian harpoons cf. Childe (1952), 36; for Heluan flints, 29.

countries and when they certainly were to a considerable extent the carriers of the archaeological materials associated with the Palestinian Early Bronze/Middle Bronze (Kenyon; MB I of Albright) and Middle Bronze I (Kenyon; MB II of Albright) periods.<sup>8</sup> One may go further and follow de Vaux (1971, 27-28) in assuming the presence of Semitic speakers to some extent also in areas where the Early Bronze civilization is attested. At least one place-name of Semitic type related to Palestine is known from an Egyptian inscription of the Vth Dynasty found at Deshasheh (de Vaux 1971, 29). Middle Bronze Age I/II practices, like the manufacture of red slipped and burnished pottery or the use of multiple tombs of cave type as family vaults, may represent the resumption of Early Bronze traditions interrupted in the Early Bronze/Middle Bronze (MB I) period. In flint work there is also apparently a basic analogy between the flints used in the Middle Bronze period and the so-called 'Canaanite' flints of the Early Bronze. However, the 'Canaanite' flint culture of Palestine and some part of Syria cannot be followed back before the beginning of the Early Bronze, and its relations to the flint culture which we assumed to have some possible connection with Hamito-Semitic speakers are entirely unknown. Early Bronze pottery in Palestine-Syria likewise represents at present a new departure. If, then, a cultural correlation between linguistic and archaeological material is to be attempted we are at present faced, on the archaeological side, with a hiatus which cannot be filled.

Attempts have been made in the past to combine linguistic and material, including archaeological, evidence in another way in order to trace Semitic origins in particular. The study of words common to Semitic languages for plants and animals, including domesticated ones, might it was thought help to determine the original home of the Semites.<sup>9</sup> In the light of present archaeological knowledge it would seem that the domestication of most of the plants and animals represented in Hamito-Semitic vocabulary took place in the Anatolian-Persian highlands and their foothills and in Palestine.<sup>10</sup> This area is unlikely to have been the home of the Hamito-Semites. These words may thus not be of as much help as was once hoped in tracing Hamito-Semitic origins. The names of domesticated animals and cultivated plants in Hamito-Semitic may, however, prove of considerable linguistic interest in another way: in the light of what is known from archaeological sources about the original centers of animal domestication and of plant cultivation and the rate of their diffusion, it may prove possible to undertake some very valuable studies concerning the diffusion of words at known rates and in known directions across the Hamito-Semitic speaking area.

<sup>8</sup> Kenyon (1966) gives a full *exposé* of this suggested correlation between archaeological and linguistic evidence.

<sup>9</sup> For a summary of various hypotheses of this kind see Barton (1934), 2ff.

<sup>10</sup> For evidence regarding the domestication of animals see Zeuner (1963); for the origin of cultivated plants see Helbaeck (1959).

Archaeological research would thus seem to have some suggestions of value to offer to Hamito-Semitic linguistics. It remains to be said that, if archaeological parallels should prove to be linguistically interesting, then external contacts between the Hamito-Semitic speaking zone and other language territories should be looked for mainly on the African and Mediterranean side and less in the direction of Indo-European than has sometimes been assumed.

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## DISCUSSION

GARBINI: What Dr. Isserlin says regarding the general development of our subject is his personal opinion. My remark is concerned with the second part of his paper, namely when he touches upon the relevance of archaeological and anthropological information for linguistic reconstruction. I think that it is not prudent to draw upon evidence from other subjects for the purposes of linguistic research. For Hamito-Semitic little work has, I believe, in fact been done in this respect, and I think that this is a good thing. It is true that in general the distribution of the Hamito-Semitic languages corresponds quite well to the distribution of early neolithic culture but apart from this very general statement we cannot say that there was any particular culture, for example bifacial flints, which could be associated with the Hamito-Semitic languages. We see that in Dr. Isserlin's paper Palestine is on such grounds excluded from the Semitic speaking region. I hope that no scholar will follow the suggestion of Dr. Isserlin and search for anthropological and archaeological facts in support of linguistic theses.



ISSERLIN: In reply to Professor Garbini's criticism I would say that, while linguistic problems must of course—and we all know this—be examined entirely on their own it is nevertheless an interesting question which is worth looking at whether, in certain cases, we may perhaps also observe a correspondence between certain facets of material culture and a linguistic community. This is of course entirely a matter which may or may not happen in an individual case and I do think, without going into the methodology of linguistic research, that it is worth while to see whether a community which bears Semitic-Hamitic linguistic traits can be isolated in other ways as well. The contribution of Professor Garbini, whose writings I very greatly respect, is based on a slight misunderstanding. I do not of course exclude Palestine from the Semitic speaking region. What I have tried to say is something different, and here I think we HAVE a problem. If we do go into the linguistic and material parallelism, the material culture early attested in Palestine could possibly be continued up to the time when we know that Semitic speakers were there. If however we do this we have the clashing fact that neolithic cultures which are POSSIBLE bearers of the same linguistic group in other regions are quite different and far more widely diffused, so that it does not seem possible so far as I can see to trace early Hamito-Semitic language bearers to the material cultures of Palestine.

#### GENERAL DISCUSSION

PALMER: I think we might first consider the first question in the terms of reference of the Colloquium, "What evidence is there for postulating a group made up of the descendants of a common proto-language?". Can we perhaps concentrate on that question.

KAYE: We know that all these languages are related, no one is going to doubt that. All you have to do is to look at prefixes and suffixes and you can see that diffusion or areal features are just out of the question. But although we know that all the languages are related, we must not be confused into believing that starred form proto-Afroasiatic gives you proto-Berber, proto-Semitic, proto-Egyptian, proto-Chadic and so forth. We know that this is not the way the family tree looks for Afroasiatic. Of course we need more descriptive work, everyone would agree that we need to know the data before we can classify. But this does not stop people in other fields—the Indo-Europeanists, the Amerindianists, and so forth, on the basis of five or six years' field work, or six or seven languages—from reconstructing. You have to start somewhere. After all people have been doing Semitic for centuries and Egyptian for just as long, and now we should start reviewing in the light of comparative and historical linguistics exactly what the family tree does look like. Indo-Europeanists have been doing this since Schleicher's days; when Hittite was discovered they modified the tree, and now you either believe in Indo-Hittite or you do not. But still that does not stop progress.



PETRÁČEK: As regards the question of terminology, I think that it is quite useless to discuss it because terminology is always arbitrary—it is the *signifiant* rather than the *signifié*. Of course there are problems regarding the relationship of the different branches, I think we would all agree on that. But our colleague Diakonoff's "Address" was also directed towards the question of methodology and I think that that is important for us. I disagree very much with him regarding this matter of method. It is regrettable that he is not present so that we could discuss it with him, but he has said that he uses glottogony for his studies. Well I believe that glottogony is more a philosophy, it is a fallacy to wish to use it for concrete studies. Naturally one must have some concept of the nature of language but we know what Reinisch did with Greek and Hamito-Semitic and all these languages when he relied on the doctrine of glottogony. Then Diakonoff also attacks the descriptivists. But I would like to point out that modern linguistics does not only consist of American descriptivism; there are many branches of structuralism—Danish, Prague, Saussurian, etc.—and there is also the development of American descriptivism towards semanticization. He has also attacked the mathematicization of linguistic studies, but I believe he is wrong because mathematics is not here the end but only the means and it can be extremely valuable, for example in classification. And how for example could we classify languages by relying on glottogony? There are some fields in linguistics where we must employ mathematical methods, not only numeric ones but also in the Cartesian sense, that is to say by giving the foundation for the elementary notions from which to build the systems *more geometrico* so to speak. It is absolutely necessary to employ such methods and we have seen the results in several of the contributions to this Colloquium. We are all agreed that a language must be studied as an entity and as a process, as Diakonoff says, but how are we to do this? It is an extremely difficult operation, but naturally it must be done. It seems to me that what is required is a 'dynamic structuralism' applying itself not only to synchronic but also to diachronic aspects, the tensions within a synchronic system, etc. And finally our colleague Diakonoff admits that he has drawn much from the Indo-European field. I agree, but this can be very dangerous if we take not only its methods but also its concepts—for instance the definition of 'sonant' in his article on the root is a definition taken from Indo-European without any analysis of this segment within the Semitic phonological system.

PARSONS: What I feel is that we must try and reach some sort of agreement about our criteria of classification, and nobody yet in this Colloquium I think has mentioned the name of Malcolm Guthrie. I think his greatest contribution to linguistics generally, descriptive as well as comparative, is an exact and scientific statement of criteria. He was not the first. My friend Professor Tucker, in his book on Eastern Sudanic I think, has a most useful list of criteria for the various language families of Africa. I do not suppose that he would stick by every word of them now, but at least he set some yardsticks by which one could judge these things. Also I feel convinced, and

I think Professor Guthrie agrees here, that there is something in language which you can call pan-African. So can we not accept that in many of our languages you have a patrilineal, if you like Noahitic, strain merging and blending with something primitive—I use the word ‘primitive’ deliberately because in a time scale African elements such as glottalization for instance are essential not only in our field but also in Ghanean languages, in Fula, and all through. I think these are the things which are of historical interest also, namely what is in the deep African structure and appears in the surface structures in so many featural ways? Finally I totally agree with previous speakers that it does not matter two hoots if X Y Z call the same thing by a variety of different names. But what is more dangerous I think is where two people use the same name for totally different features and that can lead to a great deal of misunderstanding and error, and I think that is the only type of terminology which we have to be very careful about.

ANDRZEJEWSKI: I agree entirely that we have not given enough emphasis and enough time to comparative studies in the African field. But we are faced with a very difficult problem. We have not got enough data in quite a number of areas and, although we know that some languages must be related, we do not know HOW they are related.

LESLAU: I wish that our colleagues in the field of Cushitic languages could really tell us what is a Cushitic language. So far we do not know. Take, for instance, the Kaffa dialect called Mocha, which is definitely a tone language. My question is “would a tone language be a Cushitic language?”. We know more or less what is Semitic, we know more or less what is Egyptian—I exclude Chadic—we know more or less what is Berber, but frankly we do not know what is Cushitic.

MARCEL COHEN: Our colleague Petráček has I think made the essential point—we must in our studies face up to the facts as they exist, however unexpected they may be. We must be prepared to find the unexpected everywhere. Obviously we cannot avoid the question of terminology because we must give names to the things that we are studying. But here we find ourselves entering the difficult area of collective psychology. In the case of our subject the question goes back to the 18th century when a German by the name of Schlözer was the first to speak of the ‘Semitic’ languages. By so doing he took part in an act of collective psychology, that is to say he associated a linguistic investigation with knowledge drawn from the Bible. Did he do rightly or not? That is a question that we may be permitted to ask ourselves. It would appear that, in so far as the study of the Semitic languages is concerned, the effect has been rather on the beneficial side. But following this, when wishing to talk about other languages related to the Semitic ones, the Bible was again called in—the name of another one of Noah’s sons, Ham or Cham according to how you

pronounce it, was employed and it is here that the mistake was made. For if one reads the Biblical passages in question one finds that the area occupied by Ham does not correspond at all with that occupied by what have been called the Hamitic languages. There was the real mistake, when it was decided to name 'Hamitic' all those languages which were related to Semitic and to turn them into a group. And I have spent a good part of my active life fighting against the term Hamitic and saying that we may talk of Hamito-Semitic or Semito-Hamitic—the choice seems to me to be unimportant—but we must NOT speak of Hamitic, for that does not exist as a meaningful linguistic term. On the other hand the choice of Cush was a step forward, because up till then people had talked of the 'Ethiopian languages' which led to confusion with Ethiopian Semitic—so that the term Cushitic has proved useful.

ALLAN: What I would like to see is some sort of synthesis between the thesis of sheer descriptivism without any mention of historical reality and the antithesis of historical reality based on some small degree of descriptivism—some things that came up at the very end of yesterday's discussion of Dr. Harries' generation of Berber sentences. Dr. J. Bynon had asked whether her purely synchronic description were to be considered historically at all and the answer was: not necessarily. However, it has been found that rules which work synchronically CAN also work diachronically. This is one of the points that Saussure made, of course, but that this is true has been shown for instance in the Indo-European field. I suppose any linguist would want to make use of all such information which can be got from both synchronic and diachronic evidence.

GARBINI: It has been claimed here that the task of scholars of the Hamito-Semitic languages is not reconstruction but comparison. I fully agree with this claim. But for our work of comparing it is generally stated that we are faced with five groups of languages and that these groups are interrelated. I believe that this is an oversimplification. Ancient Egyptian and Libyco-Berber are quite easily comparable, but what of the Cushitic and the Chadic groups? It would be a very great event for our comparative studies if one day we had something like a Common Cushitic or a Common Chadic. But in the meantime it is better to avoid the terms proto-Cushitic and proto-Chadic. I feel, however, that that happy day will never come—not as the result of any lack of will on the part of the scholars but due to the very nature of the material. When Dr. Andrzejewski says that the vocabulary common to North, Central, and Western Cushitic is less than one per cent we have materially no possibility of making a satisfactory phonological comparison. This may also mean that a Common Cushitic has never existed at all. The Semitist who approaches Cushitic or Chadic languages finds himself rather in the position of the searcher for gold at a river bank—from time to time he finds a piece of gold amongst a lot of sand. Professor Tucker has rightly spoken of Fringe Cushitic and I agree with him in ranging within this group not only South Cushitic but also Western Cushitic which

many scholars like Moreno, Cerulli and Greenberg call simply Cushitic. The whole Chadic group, excluding perhaps Hausa as suggested by Mr. Parsons, may be called Fringe Hamitic. If we interpret the situation from a historical point of view we may say that these fringes are the result of a historical process by which various African non-Hamitic tongues have adopted foreign—that is Hamitic—lexical and morphological features. A case in point is that of Mbugu (Ma'a), investigated by Professor Tucker. Its morphological system is completely divided between a Cushitic type pronominal system and a Bantu type verbal system. In this way we may explain the ever increasing number of Cushitic and Chadic languages. I am sure that within a hundred years there will be new Hamitic languages which at the present moment are not yet so, because they have not yet adopted pronominal or verbal systems of Cushitic type. This historical process, which we may call 'second stage Hamiticization', parallels what may be termed 'first stage Semiticization'—the intellectual diffusion of Arabic in Africa. This is a phenomenon which started several millennia ago when people from Western Asia entered Africa, both from Palestine and from South West Arabia. Cushitic, Berber and Ancient Egyptian are the result of that remote first stage Semiticization. For the purpose of comparison therefore we have to assume for all Hamitic languages, but especially for those belonging to the Western and the South Cushitic and the Chadic groups, that they are in different measure true *mischsprachen*, that is to say a mixture of African and of Western Asiatic linguistic elements which may be termed pre-Semitic. The task of Hamito-Semitic scholars will therefore be to distinguish between what belongs to the original stock of the language and what is the result of the Western Asiatic superstratum, and only the latter will be significant for comparison. Hamito-Semitic also appears to us to be very different as an entity from Indo-European. Hamito-Semitic is the historical result not only of genealogical development but also, one might even say more especially, of the progressive diffusion of one linguistic type among other types. We need therefore a methodological approach different from that of Indo-European historical linguistics, and we feel that our task will certainly not be an easy one.

CARNOCHAN: Analogy can be a very dangerous thing, and for linguistic purposes parenthood is not I think a suitable analogy, although in the world of nature we do in fact have self-propagating beings. With regard to Mr. Parsons' mention of criteria I think that this comes very close to the heart of the matter and would link up with what Mr. Wolff was saying with regard to Lukas' classification into two Chadic groups. I think that it is probably necessary to have two groups, and that the criteria for classification were probably advanced before we had sufficient knowledge to enable us to judge which were the most reasonable items to consider as criteria. If we stick to Lukas' criteria then it does not upset the classification—for instance that Bachama and the Bata languages should in fact be put into the other group if you are going to take gender and various other matters into account. If you take as a criterion the presence or absence of lateral fricatives then the Gudu language

would in fact go into the group to which this applied. So one need not necessarily be altogether at variance with the criteria, but if these criteria are applied and lead to a different grouping of the particular languages, and if it turns out for other reasons to be unsatisfactory then perhaps one will have to look once more at the groups and in the light of new knowledge readjust the classificatory criteria.

KAYE: Going back to a remark of Mr. Parsons'—he was talking about what can be called a pan-African feature, namely glottalization—I am afraid that he is confusing genetic and typological criteria. Glottalization, for example, occurs all over North America and can be shown, as Mary Haas has shown in her book *The Pre-history of Languages* which Mouton has recently published as a separate monograph, to have diffused in one genetic family after another. So its occurrence should not disturb any historical linguist. Sometimes a feature like this will diffuse, sometimes not—it does not make that much difference and should not really concern, I think, a colloquium devoted to comparative and historical linguistics. Another thing: in 1965 I probably heard the last lecture that Morris Swadesh gave before he died and he had spent some nine months in Africa doing field work, after a career in Amerindian studies, and came back and maintained that beyond the shadow of a doubt all languages in Africa could be shown to be genetically related—and there are some 1250 African languages, if we are to believe Bill Welmers. Not only that, but he criticized Greenberg's work on Central and South American Indian languages classificatorily and maintained here also that all languages of Central and South America were genetically related. So Swadesh just went a little crazy in his latter years and I think we want to avoid that in this field. Another observation that no one really touched upon: we all know that our Indo-Europeanist colleagues are just centuries ahead of us. Why is this? One of the reasons is that they have a basic common core of studies whereas we do not. People doing Afroasiatic specialize far too closely and they do Hausa their whole life and pick up a grammar of Arabic now and then. We have all picked up grammars of Galla just to see what it was like and picked up a phonetic study of Somali and so on. And this is, I think, our fault. I mean, any Indo-Europeanist who does not know Sanskrit, Greek, Latin and Hittite is not going to go very far in Indo-European studies. They all can pick up Old Church Slavic and read it, or Avestan and so forth, and know the basic facts about these languages. Furthermore they have a lot more conferences than we do, and they have a lot more people working.

TUCKER: We were asked a little while ago whether we could give a definition of a Cushitic language. I here lay myself open to Professor Leslau's criticism but I think one can by reference to a Semitic language. Two structural features which we found were: (1) the abundance of trilateral roots in Semitic languages and their relative paucity in Cushitic languages, and (2) in the verb the Semitic languages have two types of conjugation according to aspect or what you will, one type with prefixes

and the other with suffixes, whereas in the Cushitic languages you have only one type although you might have two classes of verbs, one with prefixes and one with suffixes. Of course I do not know what fringe languages do in the Semitic field, but I am putting this forward as one tentative definition. The other point I wanted to raise was the paucity of vocabulary correspondence which we have always encountered and the fact that Miss Bryan and myself have always leant very heavily on typological evidence, especially in the language Mbugu which was referred to a short while ago and which seems to have a Cushitic vocabulary with a Bantu type of grammar.

FENTON: We have heard much about phonology and a good deal about lexicon, but we have not heard so much about verbs. Since Professor Tucker did mention these I would like to make a comment. I have been very impressed by Andrzejewski's paper showing what I feel is a clear correlation between a part of the Cushitic verbal system—apparently a very minor part—and Akkadian where one apparently has an *a* vowel for a continuative tense and an *i* vowel for an aorist or a preterite, and this is in conjunction with a scheme of pronoun markers which are the same as Semitic. So there would seem to be no doubt that this particular element so isolated is very much connected with a phenomenon only as it were on the fringe of Semitic—in East Semitic, Akkadian, and nowhere else in Semitic. Since Professor Tucker has mentioned conjugations I would like to bring this up again and connect it with the Akkadian link with Berber where we are told that we have an opposition *ifarres/ifres*, which would seem to correspond to the Akkadian *iparras/iprus* type of opposition. Here we have two elements widely disparate, one in Berber and one in Cushitic, and these correspond in their different ways with phenomena which in Semitic occur only in Akkadian. Should this not mean that at a much earlier stage than we were previously willing to postulate, the wave theory obtains throughout the whole field of languages about which we have been talking? We have spoken much of family trees; I think that the wave theory should perhaps have come into it far more and that here, in widely disparate groups, we have relics of a phenomenon which is very early but which is not proto in the sense that it was not created in all areas of the groups we have been studying.

## **VIII**

### **PREHISTORIC BACKGROUND**





## THE ARCHAEOLOGICAL CONTEXT OF THE HAMITIC LANGUAGES IN NORTHERN AFRICA

C. B. M. McBURNEY

In the early days of the development of prehistoric archaeology and indeed of anthropology in general there was a widespread tendency (long since abandoned) to associate race, culture, and language. The early literature in this field from the middle and second half of the nineteenth century affords many examples reflected in the terminology in vogue at the time. Most usually linguistic terms were transferred to supposed biological entities as in 'Aryan Race' or 'Semitic Race', 'Hamitic Race' and so forth. The further assumption was wide-spread that not merely biological strain and language were associated but all manner of cultural activities which, when recognized, would serve to attest the presence and movements of the other two. Hence references to 'Celtic monuments' and even 'stone celts' (meaning ground stone axe or adze heads), 'Semitic art styles' and many others that spring to mind. All this and the underlying assumptions regarding such associations were virtually abandoned whole-sale as examples multiplied of the mutually independent spread of genes, linguistic and other cultural habits. Thus the initial point of view became rapidly replaced by its opposite, namely that there is no necessary correlation between breeding patterns, language and other forms of cultural expression.

But as sometimes happens in changes of opinion of this sort this second concept has come to dominate our thought to such an extent that it is as well to remind ourselves that although the correlation is indeed not NECESSARY it nevertheless on occasion can and does occur and that not altogether infrequently. To neglect this obvious fact, that linguistic groups, at any rate of the more closely knit kind, frequently accompany and are associated with 'traditions' in the social, economic and industrial spheres as well, would be to deprive ourselves of a valuable source of information which can, with suitable precautions, even be applied to prehistoric archaeology.

For the purposes of this discussion the most relevant aspect of prehistoric archaeology is then the attempt to isolate and recognize past human groups from the material remains of their activities rather than through surviving written records or oral traditions. The questions which arise concern the nature of the indications likely to be available, and the most important limitations on interpretation or sources

of ambiguity, which need to be borne in mind. This is of course a wide subject, and one of considerable topical interest. It has been dealt with specifically and in principle by a number of recent writers, most notably in this country by Clarke (1968). All that I have attempted in this short paper is to take a few examples, and one in more detail which falls more particularly within the scope of this Colloquium, to illustrate some aspects of the situation as I see them.

To begin with I should like to draw attention briefly to the strictly archaeological implications of two familiar cases in point, then to some of the reservations on interpretation which have been recently expressed, and finally to attempt an analysis of the evidence bearing directly on the Hamitic group of languages in Northern Africa.

My first example is that of the Anglo-Saxon invasions of Britain. Here we have an event fully attested by written sources involving the transference of populations bringing with them, and permanently establishing an exotic language to an area where it was previously unknown. How far is this event in fact reflected in the purely archaeological evidence of material objects? We see it of course in many different ways, including such elementary traits as pottery techniques, decoration, highly distinctive brooch forms and decoration, house architecture, settlement pattern, and burial practice, to mention but a few. All these combine to form a *rupture de continuité* with previously evolving traditions marked by the abrupt and simultaneous introduction of a constellation of new practices and disappearance of old. The operative terms here are of course 'abrupt' and 'simultaneous' implying wide change within a short absolute time interval and in respect of a number of different activities.

The same interpretative situation is equally evident in my second example drawn from an area closer to that with which this meeting is concerned, namely the archaeological reflection of the well known linguistic dichotomy between Akkadian and Sumerian. This is usually regarded on historical grounds as a case of an intrusive Semitic speech introduced by a new community into the area of a more ancient and long established community speaking a totally unrelated agglutinative tongue. The nature of the archaeological data on this event was vividly outlined among others by Childe (1952) in a memorable passage. He points to the detailed stratigraphic record of the older tradition slowly evolving in the spheres of ceramic technology, architectural and implied ritual practice, decorative style, abruptly broken by the simultaneous appearance of a host of new elements in this and many other fundamental sectors such as wheeled vehicles, potters wheel, radically new weapons of war and much else on a similar plane of cultural specialization. At the same time it is noteworthy that this pattern of change is also accompanied by some elements of continuity. These include religious architecture, and with it the implied ritual practices which maintain a degree of continuous development across the threshold of the differentiated activities. The whole picture is consonant with the sequence of events often recorded in history in which an incoming population brings with it a range

of contrasting customs, previously developed independently in another area, but simultaneously accepts a proportion of local traditions from earlier inhabitants in the territory of adoption. The principle for archaeological deduction is plain despite some recent disclaimers. I would go so far as to say that as stratigraphical and chronological calibration of cultural change is pursued, it becomes ever clearer that the norm is persistent trends of development by small steps over long periods of time. Sudden spontaneous re-orientation of this process within a unified community may occur, but it is notable that in a high proportion of cases that can actually be checked against historical records the element of ethnic transference remains a potent factor, despite much that has recently been urged to the contrary. To repeat, speaking from an interpretational point of view it may be argued that the operative considerations are the number, range of implication, and timing of the changes. The crucial role of this last element—the rate of change—has indeed only begun to be fully realized with the development of isotopic dating and the accumulation of viable results. There is indeed much further progress to be made in this sphere, but even so the net results are already beginning to appear and produce an impact on the interpretation of purely prehistoric situations.

What then are the facts seen in this general context which are most relevant to the question of the origins and spread of the Hamitic languages? It would seem logical to start with the known historical facts and thence work back in time to the situation documented solely by material remains, testing the ground as it were step by step.

The geographical framework, at least in outline, obviously conditions the discussion in a number of ways. At the present time languages of the group in question are (or until recently were) spoken mainly in areas of Northern Africa north of 10° north, and stretching from modern Ethiopia in the east to the Atlas range and the Atlantic in the north west. Undoubtedly the most important zone of distribution extended along the southern mediterranean littoral from western oases of Egypt such as Baharia to Atlantic Morocco. Geographically speaking this represents today a narrow belt varying from two hundred to a few miles wide, much of it extremely arid desert steppe with mutually isolated areas of greater fertility. Of these last the Atlas littoral is by far the most considerable with minor outliers to the east extending into northern Tripolitania, and the small but strikingly fertile area of the Gebel el Akhdar in northern Cyrenaica roughly two hundred miles long by twenty miles wide. The vast extent of the Sahara to the south though offering a formidable barrier to human occupation and passage is nonetheless dotted with oases which may be described as forming two main clusters. The first is a smaller cluster extending south and south east from the Gebel el Akhdar (Cyrenaica) to the confines of Egypt and bordering the Great Sand Sea to its north and east, and a second more widely scattered but much larger group which might be described as comprising a roughly triangular area delimited to the west by 3° longitude, and to the east by a line running roughly north-west to south-east from the Gulf of Gabes to Darfur. This larger swarm of

oases is obviously related to the topography of the central Saharan highlands which break the monotony of the Northern African relief in the form of an irregular ridge from the Hoggar in the centre south east along the Tummo Ridge to Tibesti and thence southwards into the highlands of Chad province and the Sudan.

Ethnically, linguistically, and culturally various provinces have been distinguished in the past throughout these areas. To the north-west, if I understand correctly, is the main group of Hamitic speakers, correlated roughly with the Atlas topographic area. Extending into the central Sahara and occupying in a general sense the slopes of the Hoggar are the groups associated with the name Touareg, while to the north-east in the Fezzan is another related group. To the south-east in Tibesti the Tebbu represent a more sharply differentiated community biologically and linguistically. In the north-east in what we might call 'the heartland of Libya' is the only area where we have relatively full documentary evidence of the continued use of Hamitic speech over the past four thousand years at least. It is this area indeed which supplies the first key or possible clue to the antiquity of the Hamitic speaking peoples. The ancient Egyptian monuments and inscriptions which provide the bulk of this historical record date mainly to the Middle and Late Kingdoms. In addition, as Bates (1914) and others have shown, to affording definite if fragmentary indications of a Hamitic tongue related to existing languages, the monumental inscriptions also give details which add up to a remarkably consistent picture of customs and practices peculiar to the speakers. These correlate well with what we know in greater detail from later writers from classical times onwards.

Among the most striking and useful of these details are certain features of dress such as the famous Libyan side-lock (reported to survive ethnographically to this day in the Sudan) and certainly widely current throughout Libya in pre-Islamic times. Another is the penis-sheath or *karnata* shown on innumerable monuments from the fourth millennium onwards in Egypt. A third in Old Kingdom and late Pre-Dynastic times was the wearing of an animal tail suspended from the belt. Another again was the painting or tattooing of conventional signs on the fore-arm, and perhaps less distinctive but extremely wide-spread, the wearing of one or two ostrich feathers in the hair often dressed in a characteristic 'bobbed' or cap-like style.

Further distinctive details may be noted in regard to the weapons of these ancient Libyans, for instance their bows clearly of 'reflex' outline, a peculiar elbow shaped club, throwing sticks or boomerangs, slings, and grooved stone-axes or hammer-heads.

Among clothes in the strict sense one notices the absence in some cases of the typical Egyptian kilt and its replacement by a long cloak, sometimes of animal pelt but sometimes also of carefully woven and decorated fabric. There are also signs of a rather distinctive style of foot wear, soft moccasin-like objects rather than the flat sandals of the Egyptians.

If there is of course nothing in all these and many other details of a like kind to link them specifically to a language group, the fact remains that they are consistent-

ly shown as the peculiarities of named peoples for whom we do possess evidence that they were Hamitic speakers. Thus collectively and to some extent separately these observations can be said to add up to a *prima facie* case for a cultural continuum which may not unreasonably be linked to the language. This case gains considerably when we compare the geographical distributions of the two. Moreover, as I shall attempt to show, we are now no longer as in Oric Bates' day confined in our comparison to geographical distribution, but can already begin to list cultural associations over a much longer measured span of time.

Direct documentary evidence for early Hamitic speakers can of course hardly be extended beyond the limits suggested by Bates, that is to say from the borders of the Nile valley to the eastern shores of the Gulf of Sirte and the Gebel el Akhdar. Beyond this to the west and south we are nonetheless fortunate in having at our disposal another important source on customs of many kinds, namely that of rock art. Finds of such art, both painted, engraved and sculptured have now been made from the Gebel el Akhdar in the east to the Atlantic coast of Morocco (Almagro 1946).

The example from Gebel el Akhdar is of special interest since it serves to link the culture of this area more directly to the well known areas of occurrence in the south Atlas, the Fezzan, the Hoggar and along the central Saharan hills south and east to the isolated groups of finds in the Gebel Aweinat and the Gilf Kebir.

Of all these the largest series and most thoroughly studied (Vaufrey 1939) are those of the southern Atlas. They comprise exclusively engravings, of which the best known concentration is some thirty sites in the neighbourhood of Figuig in southern Oran province. The majority are of wild animals long since locally extinct, such as lion, elephant, antelope, wild ass, and above all the extinct *Bubalus antiquus*. Associated with these in some cases is a distinctive often repeated design showing a ram (possibly of the domestic variety *ovis longipes*) with a curious appendage in the form of a disc with projections on its head. Human figures also occur portrayed with varying degrees of skill of which the most detailed are highly accomplished. They show clearly two forms of head-dress; undoubted examples of the Libyan side-lock (at Laghouat) identical to that of the Eastern Libyans, and a curious mass of locks projecting beyond the forehead. Both styles of coiffure are sometimes associated with clear representations of *karnata* and possibly animal tails attached to the belt (at Fedget el Kheil and Teniet el Kharouba for instance). They also frequently carry typical throwing sticks of the boomerang form mentioned above, or the still more distinctive elbowed club of tomahawk type. Bows and arrows and possibly small shields are also common, but female figures are rare (except in an apparently late and degenerate form in which they are linked by a line running from the genitals to the penis of an archer, generally in the act of hunting).

Only one of the works of art in this group is directly associated with other cultural material, namely an isolated figure of a wild ass(?) contained in a cultural layer at the cave of El Arouia. This last contains a lithic industry and distinctive style of pottery. The industry includes bifacial pressure-flaked arrowheads and an assemblage

of other specialized implement classes. The latter are all of forms typical of the pre-existing hunting culture known as the 'Capsian' so far found only in the eastern extremity of the area, namely in eastern Algeria and Tunisia. The ceramic tradition at El Arouia is of crude technique used to produce small rounded forms decorated with abundant simple incised designs, but lacking paint or slip.

In a carefully conceived piece of research Vaufrey (1939) was able to show that similar lithic material was regularly found in the immediate vicinity of all the decorated sites in this area. The same assemblage with the same pottery also occurred in stratified sites where its succession following the Capsian could be readily established. Subsequently research still in progress has now accumulated large numbers of C14 dates for this 'neolithic' type of assemblage to which Vaufrey gave the name of 'Neolithic of Capsian Tradition'.

Contrary to what was at first thought, the time span of this industrial tradition turns out to be of relatively great antiquity and duration. There is substantial evidence that it had already taken shape in the first half of the sixth millennium B.C., within a millennium at most of the latest dated expressions of the Upper Capsian. It can now be seen that this chronological juxtaposition greatly enhances Vaufrey's original opinion, namely that the technological and typological characters of the Neolithic in this territory form a remarkably continuous line of development from the earliest form of the Capsian without noticeable break to the equipment of a sheep herding and subsequently cattle herding economy with a well developed and characteristic ceramic. This continuity is in fact in strong contrast to many areas of Europe and the Near East where there is on the contrary evidence of a break at this important economic and technological interface, coupled with signs of substantial ethnic movement and/or a wave of cultural diffusion from an exotic tradition. Here in the Maghreb on the contrary the evidence is all in favour of a gradual adoption by a long established hunter gatherer community of isolated exotic culture traits, including for instance a non African strain of sheep and a particular technique of archery. The first is almost certainly and the latter not improbably of South-West Asiatic ultimate origin. The important feature nonetheless is the continuity.

This last deduction suggests a further significant corollary, namely that the 'Libyan' culture traits discussed are part and parcel of an aboriginal Capsian tradition which would now appear on the basis of the C14 readings to have been established in the area by about the sixth millennium B.C. If this is indeed the case two further problems are raised. The first concerns the origins of the Capsian; is it for instance of purely local growth or are there signs of an exotic origin outside the area? The second problem concerns the *terminus post quem* of the tradition; what other elements may have been added to the old Capsian culture by the time we can study the Eastern Libyans on the monuments of the Middle Kingdom?

As far as the latter problem is concerned the data from the Maghreb itself are neither very numerous nor very conclusive, though they have in the past given rise to some amount of controversy. Owing to their nature the Maghreb works of rock



art cannot readily be arranged in certain chronological order; nevertheless comparative study does suggest a gradation from the fine realism associated with the many wild species, domestic rams, and the most realistic human figures (including those incorporating some of the clearest 'Libyan' motifs). These come at one end of the stylistic scale while at the other are figures of very summary and imperfect execution. These last repeat some of the earlier elements but combine them with a smaller range of wild animals and with what are generally diagnosed as representations of domestic cattle. The cattle are further of interest in that they show anatomical details which link them to the type possessed by the Eastern Libyans shown on the Egyptian monuments. Indeed the strain very likely survives to some extent in the area today.

It seems likely then that the early shepherd artists were eventually superseded by cattle herders who were their cultural descendants and in all probability their physical descendants as well. Finally by all the indications of their material culture and customs which they share with the Eastern Libyans there is some reason to suggest that they shared language as well, as do their descendants.

How does this record from the Maghreb link on to that from other areas? Since the original work by French prehistorians (Flamand 1921) in the Maghreb there have been many further discoveries by German, Italian and British workers in the regions already alluded to in the south and east (Frobenius 1925; Mori 1965). In the inter-war period a large number of comparable finds were made in the Fezzan province of Tripolitania by such pioneers as Frobenius (1925) and Graziosi (1941); also by various French workers including Reygasse (1935), de Chasseloup-Laubat (1938), Monod (1932), etc. on the slopes of the Hoggar Massif and adjacent regions. The upshot was to provide a vast corpus of works both paintings and engravings, which include animals of the same kind as those shown in the best naturalistic style in the Maghreb, together with some others even more indicative of a different and more favourable climate to that of the area at present, namely hippopotamus, and crocodile. The paintings, apparently later at least in part, included representations of two-wheeled horsedrawn chariots which must certainly be later than their introduction to Egypt in the late second millennium B.C. but may well be of Greco-Roman date. Between these wide limits little could be known or conjectured concerning their date, and any effective interpretation was accordingly greatly hampered.

The situation has now been basically altered by the epoch-making discoveries of Mori (1965) over the past fifteen years. These have at last provided us with at least the outlines of a chronology with far reaching implications, to say nothing of adding virtually a new dimension to our detailed knowledge of the output of the ancient inhabitants—an astonishing variety of details of their life and customs and technology. Briefly it would appear that between the late seventh and the first millennia B.C. at least three styles of painting were practised in this area, more precisely in the Acacus Hills east of Ghat. The earliest is termed by Mori the 'round-headed style' and appears to have been practised mainly by hunters of giraffe, hippopotamus,

antelope and ostrich about 6,000 B.C. The material culture is less well documented at the time of writing but certainly included well made impressed pottery and rare flaked stone implements. This early style is replaced apparently by or soon after 5,000 B.C. by one practised by cattle herdsman, using the same ceramic forms and decoration and wielding reflex bows. They were expert weavers and the women often wore long woven cloaks. The men wore either a kilt-like garment or long cloaks with a belt. They typically wore a head-dress with a massive projecting lock standing forward from the front of the head above the forehead. Scenes actually show this coiffure being prepared, and it is interesting to note that this same curious style survives among some Hamitic speaking groups at the present, though it does not seem to have been noted among the Eastern Libyans either recently or in antiquity. In addition to their bows the men carry short tomahawk-like clubs and the women scimitar-shaped objects and small picks of apparently composite construction.

Finally there is a much simplified style associated with scenes with domestic horses, some of which may be relatively recent, and none of which can be appreciably earlier than the first millennium B.C.

Before all three stages of painting Mori places a style of engraving which as in the Maghreb appears to be wholly concerned with a wild fauna contrasting strikingly with the present and including substantial numbers of elephant, *Bubalus antiquus*, rhinoceros in addition to those species shown in the paintings. It is noticeable that a pollen diagram obtained by Mori covering the period of the paintings begins with a marked phase of *Typha* indicating conditions of substantial seasonal lakes and marshes. This was apparently followed soon after by a period of dessication followed in turn by a further period of slighter amelioration, and finally dessication leading up to the present.

Thus although the art sequence in the Central Saharan highlands differs from the Maghreb in detail, yet it shows two important points of resemblance. The replacement of an earlier naturalistic tradition by a later tradition of cattle users, both associated at least in part with a ceramic style of impressed ware and a lithic assemblage including pointed arrowheads and ground stone axes.

The earlier part of the archaeological succession in the Fezzan has yet to be revealed, but before considering the corresponding evidence for the pre-Capsian era in the Maghreb it will be useful to outline the relevant data from Eastern Libya, that is to say the territory of known Hamitic speakers. Here the cultural succession only began to appear in the early post war years and was only finally established in the fifties by the discovery of the detailed and dated succession of the Haua Fteah (McBurney 1967).

The most relevant features for the present discussion are the latter part of this succession. Both classical and post-classical eras can actually be seen in the stratigraphical record and are underlain by a well developed assemblage which lasted well into the third millennium, characterized by strikingly similar pottery to that of the Maghreb coupled with ground stone adzes, curious mace or hammer heads



of flaked stone and finally a flaked flint industry comparable in many respects to the Neolithic of Capsian Tradition, albeit lacking the geometric microliths and preserving the backed blades, burins and end-scrapers in greater numbers of the Typical Capsian. The latter in fact underlies this Neolithic exactly as just described in the Maghreb. Many other points of analogy include the ostrich egg-shell industry with characteristic decoration, and numbers of different bone tools.

Below this on the other hand continuity is sharply broken and we have instead an antecedent of a totally different character which goes back from 7,000 B.C.—the date of the inception of the local version of Capsian—to 12,500 B.C. (At the earlier date there is a further break in continuity at the supersession of still earlier tradition). It is unnecessary in the present context to enter into detail regarding the pre-Capsian stage in the two areas except to point out that there is about the same degree of resemblance as in the two regional forms of Capsian. Both have been known by the term Oranian, although in the Maghreb this term is now usually superseded by the rather clumsy term Ibero-Maurusian. What is however relevant is that recent work has now demonstrated quite conclusively that Capsian and Oranian (Ibero-Maurusian) are associated respectively with two widely differentiated physical strains. Of these the latter approximates closely to the modern Mediterranean and hence to the type of surviving Hamitic speakers in North Africa, while the former shows many idiosyncratic features which it is interesting to note recur in a similar and partially contemporary cultural context as far east as the Upper Nile.

The important deduction may be reached with confidence that at some time in the late eighth millennium in Cyrenaica and possibly somewhat earlier in the Maghreb a new culture coupled with a new human strain entered the area, and thereafter remained in possession right up to the period when their lineal successors in Cyrenaica at any rate can be positively recognized in the Hamitic speaking Eastern Libyans of the third and second millennia.

If this conclusion be allowed then it can hardly be denied that our last problem—that of the origins of the whole Capsian tradition—has a definable bearing on the ultimate origins of the Hamitic language family.

As pointed out by the writer four years ago (McBurney 1967), there are in effect three possibilities: (i) that of a local African origin in some area other than the North African coastal zone; (ii) an origin in South-West Asia; (iii) an origin in the North Central Mediterranean. We can now see that a local origin by 'stochastic' variation of the Oranian can now be rejected on the ethnic evidence alone with some confidence. Equally the geographical distribution involved almost certainly precludes any question of an origin in the Iberian Peninsula. An origin in some unknown part of the hinterland is perhaps conceivable, but totally without positive support and inherently unlikely for many reasons.

The main objections to the South-West Asian hypothesis are several; in the first place despite some suggestions by early workers in this field there is so far nothing like a convincing typological prototype, although the later blade industries in this

area are still so poorly characterized and dated that one may yet be discovered. A more cogent objection is offered by the now detailed picture of the prehistoric succession of Upper Egypt provided by the international teams in connection with the Abu Simbel project. Neither ethnically nor culturally is there the slightest sign of what must be conceived of as a major event on both counts, traversing the whole width of the lower Nile basin. Finally this hypothesis would of course imply an appreciable priority of date for the first Capsian in Cyrenaica as compared with Tunisia. Not only is there no such indication but on the contrary there is not a little to suggest that the Tunisian variant is the earlier. The present position is that two stages can be recognized in Tunisia—an early or typical Capsian and a later stage known as the Upper Capsian. Of these the emergence of the latter has now been C14 dated to the middle of the seventh millennium B.C. Some allowance has accordingly to be made for the Typical Capsian—while 500 years is not perhaps an unreasonable figure a higher one could certainly be justified, if not very much higher. On balance a figure somewhere in the eighth millennium would seem to be the most likely and it is on this basis alone that we might well reject the Asiatic origin.

We come now to the final suggestion of an origin in Italy or more directly in Sicily and the off-lying islands. Both of these areas have recently been examined in some detail and it is interesting to recall that as long ago as 1953 Vaufray with his unrivalled knowledge of the Epipalaeolithic in both areas put forward this suggestion for the first time. Since he did so much more of relevance to this issue has been discovered and published. To begin with the lithic industry: quantitatively speaking the Capsian industries it should be emphasized form a 'cluster' typologically, and as I have stressed elsewhere technologically as well, far removed from the Oranian or Ibero-Maurusian. These Capsian characters are basically present in the eighth millennium tradition of the Epi-Gravettian group in Sicily and the Egadi Islands as I believe I demonstrated in my discussion of the eastern variant four years ago (McBurney 1967). Two other major characters can be linked to these affecting both the art and its content both sides of the straits separating South-West Sicily from Cap Bon. Stylistically the art is characterized by a high degree of naturalistic representation. Human figures equally naturalistic also form an important element often shown engaged in ritual. Among particular designs we also have curious bird-headed and trunk-like masks seen most notably in the dancing scene at Addaura near Palermo, and recurring in the South Algerian group of sites of the Monts Ksour at Moghar-Tahmtani. Another motif is that of tiny running or standing figures associated with very large animal figures not unusual in the Algerian series and clearly seen also in the dated Egadi series. An unusual and particularly striking feature directly associated with the earlier Typical Capsian are the sheaves of engraved parallel lines known as *traits capsiens*; these have recently been established as a regular feature of the Italian Epi-Gravettian. Finally we have the presence in the Later Epi-Gravettian of pebbles painted with broadly drawn parallel strokes. Identical specimens dated to the mid-seventh millennium occur at the Haua Fteah in

Cyrenaica and roughly pointed designs though less well preserved are also known in Capsian sites in Tunisia. If all these and other details are now related to the absolute chronology we can see that the long established Italian culture offers a very viable prototype for the whole Capsian complex. There remains it is true the problem of the physical separation by sea. Sub-marine contours show that at the height of the last glaciation the gap between Sicily and Cap Bon would have been narrowed to the order of 15 to 20 miles, and the two coasts would certainly have been in sight on a clear day. At the time of which we are speaking the separation would have been more nearly in the order of 40 to 50 miles, but it is worth noting that there is clear evidence from their distributions that both Epi-Gravettians and North African Epipalaeolithic populations were already in possession of some degree of navigational knowledge.

All things considered then my thesis is that the Hamitic speakers of four to five thousand years ago were the lineal descendants of a powerful group of mediterranean physical type in the technical sense, who entered the area some nine thousand years ago stemming from Italy and the south Italian islands, making their first landfall in Tunisia and spreading thence east and west along the coast and southwards into the central Sahara at least by the end of the seventh millennium.

If in all this I have said virtually nothing of archaeological evidence bearing on the possible further spread to regions south and east of the Sahara, it is quite simply that we so far lack the necessary chronological and industrial data which make the conclusions offered in the north at least reasonable. That the cattle herding element which characterizes the later stage of the Neolithic of Capsian Tradition may have penetrated the north from this quarter, and even conceivably have originated in Arabia entering Africa across the Bab el Mandeb Straits, was urged by Rhotert as long ago as 1952. Although at the present the theory lacks positive support at almost every point it may be remarked that it is still not inconsistent with what I have said above. That the language also spread from that direction I would find hard to accept in view of the general pattern of cultural evolution and diffusion I have just presented. I would rather think in terms of a complementary spread of artistic ideas from north to south than *vice versa*.

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## FINAL DISCUSSION

CROSSLAND: In this final discussion I suggest that, since we had the archaeological or prehistoric papers last, we might commence with any questions or comments people would like to offer on those, then go on to Hamito-Semitic linguistics, and finally have an opportunity to raise matters of general method.

PETRÁČEK: A few years ago we published in Czechoslovakia a two volume History of Africa, unfortunately in Czech, the fourth chapter of which was devoted to the linguistic prehistory of Africa. There I dealt with essentially these same problems, drawing on the work of Dr. McBurney and others, and the conclusions I came to were more or less the same as those reached by Dr. McBurney. In discussing the problem of the Berbers—or proto-Berbers if you prefer—I expressed the opinion that they were already there by say the 10th millennium B.C. If we accept the correlation of the ecological, anthropological and archaeological data with the linguistic, we have to admit that the proto-Berber language must have come from the East along with the Mediterranean type and the Capsian industry. But then we have to ask the question 'Where was the primitive centre of these Mediterranean bearers of the Capsian industry?'. And here I would like to ask Dr. McBurney about the Capsian in the Arabian peninsula. Because we also find a variant of the Capsian if I am not mistaken—the Khargan, in Somalia. So what is the situation in the Arabian Peninsula? Naturally I do not want to suggest that the proto-Hamito-Semites came from that region, but perhaps they were present in this Syro-Palestinian and Arabian area. If we accept for argument's sake that the situation was as it has been described, namely that the Berbers were in this region between 10,000 and 5,000 B.C., then from

this period can we assume a stage of dessication of the region? And if we accept that, then it only remains to assume the diffusion from this dessicated zone of the populations to the North and the South—the Hausas for instance. So there are our two principal problems: that of the Caspian in the Arabian Peninsula and that of the dessication of the region.

ISSERLIN: I am sure that we have all found the *exposé* by Dr. McBurney extremely stimulating. It also raises a number of questions in a more acute form than has been done before. The main problem which I see is this: Do his findings in Libya, with a side-glance at Algeria and Tunisia, provide us with an entity which is of wide enough general importance to be perhaps of pan-Semito-Hamitic interest? Now I would like to go back to one or two points. He mentioned anthropological material and he referred to the well known preponderance first of Crô-Magnon and then of Mediterranean types, and he referred also to the occurrence of Mediterranean types in early Palestine. There I would like to make a first query: the Mediterranean type includes various subtypes and, for instance, the earliest Palestinian Mediterraneans—the Natufians—are perhaps not immediately comparable to everything later in North Africa. In addition, Mediterranean types have been shown to extend well beyond the Semitic speaking area in Mesopotamia. With reference to blood groups, which should perhaps also not be left out of account nowadays in addition to skeletal material, I would only refer to the distribution maps in the standard work: A can be said roughly to fringe the Mediterranean and go into Asia, B is more of interest to the Hamitic area—if we dare call it that—but goes too far into Africa as well, O is very interesting for us because it includes what you might call Peninsular Arabia and more or less the Libyan, Berber and partly Early Egyptian region, with an extension for instance into Southern Spain. The three do not give the same impression but one at least is interesting. This we should keep in mind. Now when we come to the matter of purely cultural material we are unfortunately, so far as I can see, faced with a number of difficulties. It would for instance be very nice if we could derive the culture of North Africa from Arabia and Palestine or *vice versa*. So far as I can see at the moment we cannot do this. If we begin in Palestine, the earliest Mesolithic culture is the Natufian. So far as at present known it extends slightly into Egypt. There are scattered occurrences of some technical features like the Helwan flaking elsewhere, but so far as I can see no prehistorian up to now has said that shall we say the stone age culture of early Sudan or Tibesti or Libya is exactly the same as the Natufian and can be derived from it—they are different. I have just had the good luck of talking to Dr. McBurney and he confirmed an impression—I hope I report him rightly—that the Central Sudan zone (Tibesti, the Central Sudan, across to what used to be the Anglo-Egyptian Sudan, Shaheinab and thereabouts) belong to an interrelated second zone which is indeed kindred to the Libyan zone but not quite the same. Then we get the finds in Arabia, which are still few, but there is enough now to create some difficulty. We have a number of sites round the Rub' al-Khali

which by an authority who ought to be taken very seriously have been regarded as akin to the again Sudan and East African zone. They are certainly, so far as I can see, different from the earliest Palestinian material, even from the second, Tahunian, culture of Palestine which is the classical pre-pottery Neolithic—this, just to make things a little more difficult, is now known in the Qatar region. So what is one to do? Which if any of these are the people to be associated with early Semitic? If you take the Natufian in Palestine you run into difficulties elsewhere. If you take the Tahunian, you have indeed analogies in Arabia but you do not get any really good parallels in Sudan. If you take the Sudanese and Libyan materials then you get a nice parallel in Arabia but you are not really quite on the rails with Palestine. This is not, I feel, the end of the difficulties. When we get near historical times we have the what we may call West Semitic and the East Semitic areas. Now for West Semitic from about 3,000 B.C., and certainly 2,000 onwards, you can find a reasonable archaeological parallelism, always provided that you do not press the case. You must not say that every pot and every flint must have been wielded by a Semitic speaker. Just as—to take a very crude example—you could say that the samovar is largely distributed in the Russian speaking area but by no means all samovars are used exclusively by Russian speaking tea drinkers. They are not. Still, by and large the parallel is a reasonable one. Now unfortunately—and always assuming that this archaeological and linguistic parallelism is meaningful, and it seems to me that it may be so regarded—you have two further difficulties. The first is that what goes on about 3,000 B.C. cannot, as far as I can see, be linked with what is before, there is a gap. The second is that you cannot easily link it with the East Semitic area, except possibly by taking Dr. Waechter's very badly reported flints which are supposed to be similar to the Arabian ones or, and this is a counsel of despair, referring to certain similarities in the Urug culture and West Semitic—but it is a counsel of despair because Urug clearly has primarily nothing to do with the Semitic expansion. So it seems to me that, at the moment, we are still left beyond a certain point without an answer. We have certain cultural areas if you like associated with a wide area which can be called a Neolithic continuum which shows certain similarities, but also divisions from a very early time. And perhaps this is as it should be. If, for instance, you think of the idea which has been very interestingly proposed by Professor Garbini of influence from the Semitic speaking area into what is later called Hamitic, if we think of a cultural continuum with slightly varying, if associated, components affected not only perhaps by influences from the Palestinian region but also by influences originating all over the place. For instance you could think of the Tibesti region drying up and people from there pushing off into the more favoured regions in the margins—this would be a possible carrier. You could think of the trans-African trade route which is known quite early, the Nile and again the Tibesti region. One could make out a number of such possible paths of diffusion, and if you thought of a continuum which we have prudently left undefined in which various influences go in various as yet uncharted directions, then perhaps for the moment that is the best which I think we can do



until further research gives us more enlightenment. I would be very grateful to know what Dr. McBurney thinks of the points which I have put.

CROSSLAND: Before Dr. McBurney gives us his comments I would like just to voice two questions myself. First of all I wonder if he could say a little more about the possible significance of these similarities between Capsian in Libya and cultures in South Italy and Sicily and the relative chronology if it is clear and what this may mean for our ideas of migrations or continua in North Africa. And secondly to Dr. Isserlin: I wonder if the problem can be met at all, or one possible hypothesis is, that Palestine in the Natufian period was culturally aberrant—out-side the proto-Hamito-Semitic continuum, if there was one—and perhaps an area which adopted Semitic speech rather late. Can one perhaps get a pattern in that case?

ISSERLIN: I think it is one of those cases you can neither prove nor disprove at the moment. It has been stressed by researchers in other fields such as the late Professor Noth that till about 3,000 and even after there is a great deal of obviously non-Semitic ethnic material as shown by place-names in the Syro-Palestinian littoral region. It would be possible to assume that these earliest people were non-Semitic speakers. One feels a little hurt about this assumption, one does not like it, but one might have to face it—if one assumes that Arabia in this case was the real region. The other possibility is fringes such as the sand dunes and the Negev Desert where I have a feeling that one or two arrowheads of the kind which are referred to may have turned up, but one or two arrowheads do not yet make a Semitic migration.

MCBURNEY: Some very alarmingly complex issues have been raised by the previous speakers and there are a number of points here that I do not really feel competent to answer. I am afraid I am not familiar in any detail with the material from Central Arabia. The difficulty is that it is *prima facie* very hard to imagine a current entering from Arabia and passing westwards through Egypt into first Eastern Libya and then on into the Maghreb. In the first place you have the apparent contradiction of the available structure of the C14 dates; you certainly cannot think in terms of a Natufian passing this way. But I am not quite sure that this is quite the difficulty that it appears to be. If we leave on one side the structure of the carbon dates to start with I think that one must be careful how one conceives of entities such as the Natufian, that there is a real continuum running roughly from the Nebk Valley in Syria—that is to say roughly the latitude of Damascus—southwards into the southern limits of Palestine down the coastal strip, I am fully prepared to admit. This it seems to me is a real rounded cultural entity. The very faint traces of it that we find in Helwan that have been repeated from work to work until they have become almost legendary—and when you look at them they are all contained in one little box about so big—I do not think that these really constitute a very massive barrier to possible movements. I think in the first place one must remember that in Palestine the real break

does not occur with the arrival of the Natufian. Archaeologically speaking to my mind the real break goes back about at least four or five thousand years earlier when the indigenous Palestinian Upper Palaeolithic does indeed undergo a major change, and you have a remarkable spread not only in Palestine but right down into the central areas between the Tigris and Euphrates valley and the Mediterranean coast. Some years ago Dorothy Garrod did a reconnaissance in this area along one of the pipe lines that runs right across this area from Irak to the Mediterranean coast and there she found what was apparently a Late Upper Palaeolithic. Now it is normally said that such cultures as the Zarzian and some of the cultures recognized by Rust at Jabrud and so on did not make their appearance in Palestine until at the earliest around about 11,000 or 12,000. Now this would be immediately before the Natufians. But in fact if you examine the evidence in detail there is considerable latitude here and I am inclined to think that what one might call very broadly, if I may be excused a loose term, Gravettoid industries, that is to say industries with a whole architecture comparable to those that you find in Italy or indeed in Russia or indeed, as I have recently been studying in North Iran up in the Caspian, in this area you find a great nucleus of cultures which make their appearance in Palestine I think no later than 12,000, but I would not be unhappy to find they were as early as 15,000, despite what has been said to the contrary. Now these peoples would then have spread down, or could have spread down, through the steppic areas a considerable distance to the south. So I think that one ought to think, in the meantime it is wise to think, of the Natufian as a superstructure on a much wider spread substructure. But this still leaves you with the difficulty of the carbon dates. Now I am sure that a great deal will turn on properly dated Epipalaeolithic sequences from Lower Egypt, and this is just what we lack at the moment. There simply are not the finds. There are indeed backed-blade industries; on the whole they look to me more like Nilotic derivatives but I do not know. Professor F. Wendorf announced in 1967 these discoveries from Middle Egypt and when he publishes them in detail it will be very interesting to see whether they fill this gap. But I still think that it is extremely hard to see the origin of this cultural watershed that seems to be so well established in the area and to give rise to the Capsian and, following it, the Neolithic-of-Capsian-Tradition to reverse the flow and bring it right across Eastern Libya. It does not seem to me to fit the available data at all. Now there may be some mistake in the available data but there is nothing to suggest it at the moment. There is neither the prototype, nor is there the chronological structure to suggest it. I think another point that I did not have a chance to make and which has a bearing on this is the development of the Neolithic-of-Capsian-Tradition. Now I believe that this does represent a real spread. I would be inclined to think that it was the earliest pastoralists in the western Maghreb who submerged surviving relict populations of the Mechta el-Arbi type. This is the way it looks to me. We could do with much more detailed evidence, we could do with much more abundant dates, but this is the simplest pattern into which I can arrange the data at the present time. As to the connection



with climatic changes, this is exceedingly intriguing. The fact is that we do not yet have a detailed climatic succession for North Africa say for the early post-glacial. We can see that there was a dry oscillation. I think it arises from Higgs' mammalian work that there was a dry oscillation somewhere between 10,000 and 12,000 which affected a good deal of North Africa. Subsequently the climatic evidence from the mammalian fauna is submerged once the Neolithic spread starts to take place, because the natural biotype balance is totally upset by the goats and the sheep and the cattle, so it is very difficult to say; a pollen sequence would probably help a good deal. But I did notice when working in Iran that there is very evident traces of the Allerød and even the minor climatic fluctuations with which we are familiar in Europe—going back: Younger Dryas, Allerød, Older Dryas, Berling, Oldest Dryas—we seem to have successions of these as far to the south and east as Iran so why not in the Mediterranean? They could have played some part but WHAT I cannot see at the moment.

ISSERLIN: Just one point, arising out of the answer we just got, which I feel ought to be made again about the Gravettian. This of course, as Dr. McBurney says, in itself is a candidate. But I fear if we bring it up we run ourselves into other difficulties. First of all, the length of time involved. If we deal with a thing 5,000, 7,000 years B.C. I think we are still reasonably within the realm of where we can guess. If we go back 14,000 years, we are going rather far back. The second thing which troubles me more is we will then be dealing with a cultural entity which embraces vast areas well outside the known Semito-Hamitic region—Russia, even parts of Western Europe. Are we to assume this? If we do, we find that we have run ourselves into new difficulties I think.

TUCKER: I hope that I am not being too naive but I gather that the Capsian civilization we had described here was a pre-pottery civilization, but in the History of Africa written by Oliver and Fage, the proto-Hamites are claimed to come from the Caucasus area and to practise a dimple-bottom pottery civilization. I would be very interested to be set right on this particular point.

MCBURNEY: Just briefly on the last point. I am afraid, with due apologies to my old friend Professor Oliver, that I really cannot conceive that the presence of dimple based pottery in East Africa has any very close bearing on this problem; (a) it is much too late, and (b) it is much too generalized and isolated a culture trait, I should have thought. The Capsian is certainly pre-pottery, there is no doubt about that at all. The suggestion that the place of origin might be in the Caucasus is new to me and tremendously interesting I should have thought.

MARCEL COHEN: In placing on record the success of this Colloquium I would like

to praise its organizers on all points. Although I do not think that we can draw any great linguistic conclusions from what we have done, we have seen that the number of workers employed in the field is growing rapidly and this I think holds out for us high hopes for the future. I would like to add only one or two words to what has been said. We have spoken of the drying up of the Sahara desert and of the extraordinary consequences that this must have had upon its populations and the languages spoken by these. But there is another reservoir, another desert that we must think of, namely that of the Arabian Peninsula. Arabia is almost deserted, it has a very small population and has been very little explored—much less so than the Sahara. We may therefore hope for some progress from that quarter. To return to the Sahara, we must mention the rock paintings which have been found and which bear such an extraordinary resemblance to those of the Bushmen. That suggests migrations, movements of peoples that we can hardly guess at at this stage. And then there are the megaliths of Southern Abyssinia, comprising dolmens similar to those of Europe and menhir type statues. No local tradition has been recorded to date which would give us a clue as to the civilization and period to which they should be attributed. I think that, in addition to language, we should attempt a comparative study of customs and beliefs. We should try to determine whether those peoples who speak Hamito-Semitic languages are also distinguished from their neighbours by other cultural features—I think that one could say something about their material culture, dwellings, costume, etc. There is also the question of beliefs and religions, the opposition between polytheism, animism and monotheism. I will not go on, but I think that there is a whole series of studies to be carried out in addition to and alongside our linguistic studies.

GARBINI: I want to add just a few words about a question raised by Professor Cohen, namely the megaliths of Ethiopia. I think that perhaps few people yet know that in the Tihamah in the Yemen there have recently been found some megaliths of the same type. They were discovered by two Italians, a doctor and an ambassador, and are being published in the journal of my Institute. In addition some other megalithic monuments—dolmens etc.—have been found in Central Yemen in the region of Mosna.

VYCICHL: The Berber population of North Africa is not an ethnically homogeneous entity. We can distinguish different types which are not mingled but which still today occupy distinct areas. There is one group, brachycephalic, which lives in the Djebel Nefusa area, Djerba and the Mزاب region and this population would appear, according to the anthropologists, to be distinct from the Mediterranean type. This latter is the type of the Ancient Egyptians and of the Libyans who were called TEHENU, and is also distinct from the tall, fair, blue eyed Libyans, the centre of whom is in the Rif, among the Chaouis and sometimes even elsewhere—for instance in the Fayum where there are villages where people are red haired and blue or green eyed

and who are not Egyptians. Certainly we must not attribute an exaggerated importance to external physical aspect, but when we see for the first time the blond TEMEHU Libyans shown in the tomb paintings of Seti I, we see that they are differently clad; they have long coats, they are tattooed and they have a different hair style from the Libyans and that means that they belonged to a different civilization. When we study the Berber languages we see that the whole of the grammar, of the grammatical elements—pronouns, verbal suffixes, prefixes, nominal suffixes—are closely related to those of Ancient Egyptian and of the Semitic languages. We even have some names of the parts of the body and a few other words—but not very many, I do not think that the number will exceed much more than two or three hundred—the bulk of the vocabulary is not Semitic and it seems probable that this vocabulary should be attributed to the other groups which in later times were to make up the Berber ethnic complex. Perhaps it will be possible one day to distinguish several layers of language and to attribute for instance the word for ‘cloak’ (*afaggu* in Schilha) to the tall population who wore cloaks ... because the Tehenu Libyans, who were Mediterranean, had no cloaks but were practically naked—they had only the penis sheath and an animal tail, they could not have had a cloak. I think that collaboration between the prehistorians and the linguists might prove extremely fruitful and help solve the problem of Berber ethnogenesis.

CARNOCHAN: We began this meeting on Wednesday with I suppose what might be described as ‘a kick in the pants’ from Professor Diakonoff, who stressed that not enough was being done and that we should in fact take ourselves to task over this. During the intervening two days we have learnt that a certain amount is indeed being done in the various fields—that we have in fact got groups of scholars working perhaps in areas unsuspected by some of us. It has been very informative to many of us here to find out how many projects are being pursued by different groups in the various parts of our area, and I think that the information which has thus come to light will prove extremely useful.

CROSSLAND: I did suggest at the beginning of this final session that a third set of topics or questions which we might be interested in were matters of method, general considerations in comparative and historical linguistics. I think as my Chairman’s summing up, as far as I am competent to give one, I will venture to suggest what some of these questions seem to me to be. It is obvious to those who work in other comparative fields, and I think almost a truism, that comparative Hamito-Semitic linguistics shows us in a classic or extreme way problems of large scale large area language comparison under certain conditions—in particular the difficulties which one meets in comparing languages which are attested at very widely ranging periods. In that, Semitic and Hamitic taken together are in a more extreme and more difficult situation even than Indo-European, since your first recorded Semitic language is Akkadian late in the third millennium and many of the languages subject to com-

parison are only known as spoken idioms today. Dr. Kaye referred to the virtues of Indo-Europeanists—that we had got ourselves well organized and had better ideas of method and so on. I think that though I am one I confess that this is to some extent a matter of luck. First of all we have been at it a little longer, though a new discipline could easily have overhauled ours I think; more important, we enjoy certain advantages in the material in the languages we deal with: they are known over a somewhat smaller time scale, from in the extreme say the 16th century B.C. in the case of Hittite, down to today; and, perhaps more important, many of the most important members are known from literary sources—pretty good ones—over a relatively long, conveniently long, period: your Italic languages from 500 B.C., Greek from 1400, to the present day, Germanic from early in the Christian era. Moreover, many are in what I would call near phonemic scripts and anybody who has dealt with cuneiform or Egyptian will know what an advantage that confers. Nevertheless if Indo-European can serve as a sort of model or encouragement, fine. Hamito-Semitic in reverse I think shows a number of interesting problems for those working in other fields. Of those which have struck me and which might be worth further thought, one is the importance of morphology as against lexicon as a criterion for relationship and what for convenience we call degree of relationship. And tied up with this the validity of glottochronology: how far one can validly estimate, from similarities of vocabulary, how long apparently related idioms have been diverging. I must admit that I am sceptical about glottochronology from the results I have seen yielded in Indo-European, and I would by the way take issue with the opening sentence in Professor Vergote's paper which, if you will excuse my French, reads this way: "Dans le domaine de l'indo-européen et des langues romanes, on détermine la parenté et le degré de parenté entre les différents idiomes en se fondant essentiellement sur les faits de vocabulaire". Now that may be true for Romance comparative linguistics, it is not true I would say for the practice and approach of Indo-Europeanists as it still is at present. I do not think the application of areal linguistic methods based primarily on vocabulary comparisons, as done by Porzig for example, has been very successful in yielding a plausible pattern for the differentiation of Indo-European, both geographically and in its phases. And if one attends more to morphological similarities or differences, particularly in the verbal system, the medio-passive for example, I think one gets a different pattern of development and differentiation and to me a more plausible one. This is a lively issue within Indo-European and I am not suggesting that what is adopted for Indo-European is valid for other language groups, but I think the analogical argument in Professor Vergote's paper is not a sound one. If it appears that Hamitic studies and studies of possibly related African languages do yield us some test of validity for glottochronology this will be excellent though I find it difficult myself to see how; one of the problems I think is the time scale mentioned. Some defenders of the method mentioned say it is right over two thousand years or so. One answer to that is: we want evidence over much shorter periods for certain purposes and it seems not to give it. But in

any case I do not see how you can easily test it over these longer periods except in the very rare case where you have got a group such as Semitic, known from the third millennium B.C. in the case of Akkadian, down to the present; or if you can assume firmly from archaeological evidence a differentiation of branches of a language family at some fairly tightly fixed date in prehistory. Another interesting question raised which I hope will be investigated further is that of typological similarity or even similarity in deep structure perhaps implying genetic relationship. People seem able, if I have heard right, to say we feel this language is African and that they have this feeling and accept it as valid even in the case of some languages which have so far been classed as Hamitic or belonging to the broader Hamito-Semitic family. If this can be made objective it will be an excellent advance. As regards substrate phenomena raised in one of the last interventions, it seems to me that what one needs to do to assume a valid substrate is to be able to work out some structure for it within itself, within the language in which you think you can observe it. And finally I would like to refer to the to me very interesting report of Dr. Bender of his objective methods or attempts to decide mutual intelligibility between dialects. If this can be made a hard method to be used on informants we shall be well situated, or better situated, to decide what we call a language and what a dialect in dealing with modern languages.

I would like to end by offering MY thanks and congratulations as Chairman of the Historical Section of the Linguistics Association, which was involved simply as providing the place where the gleam came into certain eyes about this meeting and the first steps towards organizing it were taken. One cannot praise highly enough the hard and well calculated work of Drs. Theodora and James Bynon in getting all this arranged and off the ground today, and as one of the visitors I would like to give thanks only in the second place after that to the School of Oriental and African Studies for being our hosts and all its members who have helped us, particularly Mr. Carnochan.

J. BYNON: I would like to add our thanks to our Chairman and would also like to thank most warmly our Section Conveners—Professor Barr, Mr. Carnochan, Professor Palmer, Dr. Andrzejewski—and finally I should like to thank all of you who have come here to participate in this first colloquium devoted exclusively to Hamito-Semitic comparative studies and most especially Monsieur and Madame Marcel Cohen who have made so considerable a journey in order to be with us.



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